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HYPOTHESES ABOUT THE ORIGIN AND DISAPPEARANCE OF MATTER *

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1. *Introduction*

Let me begin by setting down in tabular form all possible hypotheses about the origin and disappearance of matter.

(*A*)	(*B*)
Hypotheses about the duration of matter and energy in the past	*Hypotheses about the duration of matter and energy in the future.*
(*A*1) All matter and energy has existed for all time.	(*B*1) All matter and energy will continue for all time.
(*A*2) All matter and energy has existed for approximately the same time, which is the time that has elapsed since the date of the Creation. According to this hypothesis no particle has existed for a longer time than has elapsed since the Creation began nor for a shorter time than has elapsed since the process was completed.	(*B*2) All matter and energy will continue to exist for approximately the same time, which is the time that will elapse until the End of the World. According to this hypothesis no particle will continue for a shorter time than will elapse up to the beginning of the End of the World nor for a longer time than will elapse until the process is completed.
(*A*3) Any particle of matter or quantum of energy may have existed for any time. This is a way of saying that matter and energy are originating without cause, continuously, at random, and not as a result of anything in the existing state of affairs.	(*B*3) Any particle of matter or quantum of energy may cease to exist at any time. This is a way of saying that matter and energy are disappearing without cause, continuously and by extinction, at random, and not as a result of anything in the existing state of affairs.

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Any hypothesis in the left-hand column can be coupled with any in the right-hand one. The nine combinations thus obtainable cover between them every logical possibility concerning the total duration of matter. When one couples (*A*1) with (*B*1), (*A*2) with (*B*2) or (*A*3) with (*B*3) one obtains a symmetrical hypothesis. Only these three together with the unsymmetrical combinations of (*A*3) with (*B*1) have their supporters and call for critical study.

On this occasion I propose to consider first the hypotheses about the duration of matter in the past and then those about its duration in the future. It will be found that all of them except (*A*3) and (*B*3) have to be rejected for various scientific reasons. Unless one decides to form no opinion at all about the duration of matter one has therefore provisionally to adopt the one according to which the period of existence of a particle of matter or of a photon is indeterminate both in the past and in the future. This is not, be it admitted, because the hypothesis of the indeterminate duration of matter and energy can be proved valid. It is because this is the only one that no one has succeeded in proving false, the only one that can survive critical study. It is reached by a process of elimination. A scientist may with good reason suspend judgment; but it is not legitimate for him to adopt any other combination than (*A*3) with (*B*3) unless he can (*a*) overcome the scientific objections to his choice and (*b*) provide a positive justification for it other than that of pure faith.

2 *The hypothesis of continuous existence in the past*

This is an appropriate description of (*A*1). It has to be rejected for the simple reason that a number of observable processes in a self-contained system tend asymptotically towards a limiting condition. If the hypothesis were true every asymptote would have been reached by now and the universe would be very different from the one that we live in. This follows from the fact that many physical processes are irreversible. So the universe must either have existed for a finite time (*A*2), or it is not a self-contained system (*A*3). This point can best be explained with the help of a few examples.

(1) During every energy change in a self-contained system the total potential energy in the system decreases and the total kinetic and radiant energy increases by the same amount; a self-contained system being one into and out of which no energy or matter passes. This is no more than one formulation of the second law of thermodynamics. Another formulation is to say that the entropy in a self-contained

system tends asymptotically to a maximum limiting value. Both statements mean the same thing. If (*A*1) were valid there would be no more available potential energy ; the limiting value for entropy would have been reached by now ; no energy changes could occur. So one must conclude that (*A*1) is not valid. Either the time during which energy changes have been occurring is finite, as is asserted by (*A*2), or the universe is not a self-contained system, as is asserted by (*A*3).

(2) The movement of all bodies in a field of force is a one-way movement. The direction is from a position of higher to one of lower potential. If one does not limit the meaning of the word ' falling ' to movement towards the centre of our earth but allows the word to stand for movement in the direction of any field of force it is quite precise to say that bodies moving in a field of force are always falling. They go on falling until they lodge on a place from which they can fall no farther. During the process they gain kinetic and lose potential energy ; they conform to the second law of thermodynamics. In fact the law according to which movable bodies always fall as far as they can is but one particular illustration of the more general second law of thermodynamics ; it is really implicit in what was said under (1) above.

Had all matter and energy been in existence for all time every object would, by now, have fallen as far as it could. The asymptote would have been reached for which no movement in the direction of a field of force was occurring. But in fact about half the matter in the universe exists in interstellar space and is still in process of falling on to stars ; on our earth rain falls from clouds and the water of our rivers falls down inclined planes into the sea. By this special application of the second law of thermodynamics the bodies that are still falling cannot have been doing so for all time. Each of them must have come into existence a finite time ago. Once again (*A*1) is ruled out.

(3) Eddington proved mathematically that an Einstein universe could only both contain matter and be stable if it were either expanding or contracting. Observation of distant nebulae shows a shift of all the lines of their spectrum towards the red end. This is spoken of as the red shift. It seems to be proportional to the distance from us of the observed object. Its simplest interpretation is that of Doppler effect, and if this is correct it shows that the nebulae are receding from us with a velocity proportional to their distances. So the red shift has

been interpreted as observational confirmation of theory and as proof that the universe is expanding. One can only refute this conclusion if one does two things. One must find (*a*) a substitute for the Einstein universe, such that it would be stable without expanding, and (*b*) an alternative explanation of the observed red shift. The attempts at both made so far amount to little more than surmise.

If the whole of the matter and energy in the universe have existed for all time and the expansion has been unidirectional the density of matter in space must have reached asymptotically the value zero by now. But it is not zero. So, yet again, those who accept the hypothesis of an expanding universe must also reject hypothesis (*A*1).

3 *The hypothesis of past finite existence*

This is an appropriate name for (*A*2). It accords with the theological doctrine that an Act of Creation was performed once, and once only, in a remote past, differently dated, in thousands of years by some, and in millions by others. The reason why it has to be rejected is not, as for (*A*1), that it is incompatible with undisputed scientific facts and laws. The reason was hinted at when I first discussed this hypothesis in 1940 and gave it the name ' Once-upon-a-time theory'. Like a fairy tale it fails to conform to the principle of economy of hypotheses.

(*A*2) asserts that in the whole of infinite time two moments have had a unique significance. One is the commencement, the other the completion date of the universe. During the interval between these significant moments one is asked to believe that the most fundamental laws of physics, the nature of material systems, those features by which we recognise matter and energy when we encounter them, were different from what they have been ever since. ' Once-upon-a-time ' hypothesis (*A*2) asserts by implication : ' the law of conservation of energy was suspended; all sorts of things happened that could not happen now. That was during the time, which may for all that we know have been very long or very short, between the moment when the first proton appeared in our universe and the moment when the very last one arrived in its allotted place. But immediately after that happy event all the laws of physics were imposed with the rigour that they have now. And they have never been suspended again.' It is alien to the spirit of science to postulate conditions of that sort. The mildest comment that can be made on (*A*2) is that physicists do not have a place in their science for events of which the type does not repeat itself.

4 *The hypothesis of continuous origin*

This is an appropriate name for (*A*3). When I first advocated it in 1940 in my book *Science versus Materialism*,[1] I called it 'The At-any-time Theory'. The word 'any' hints at the absence from the all-too specific hypotheses implied in (*A*1) and (*A*2). The same hypothesis has since been independently advocated by Hoyle,[2] and and by Bondi and Gold,[3] with much more supporting evidence than I provided. It has been supported by McCrea.[4] Their contribution was to formulate it in mathematical language and present it in quantitative terms. They were led to undertake this task by two considerations. Firstly, extrapolations by which to arrive from various different observations at a date for the Creation that is implicit in (*A*2) led to conflicting results; secondly, neither (*A*1) nor (*A*2) could be reconciled with a principle known as 'The Perfect Cosmological Principle', according to which, apart from unimportant differences, the universe presents the same aspect at all parts of it and at all times. The conclusion reached by such distinct lines of reasoning have been received with scepticism in some quarters, but never disproved.

5 *Disappearance by extinction or by removal?*

If new matter has been originating for all time it is necessary to explain why we do not observe an infinite amount of it. In other words the origin of new matter must be balanced by the disappearance of old. And the disappearance must be genuine, absolute. It cannot, of course, consist in the conversion of matter and/or energy into something else. Nor can what has disappeared leave any trace. If it did we should observe an infinite accumulation of traces. So much is agreed by all those who support (*A*3). But two quite distinct assumptions have been made about the manner of the disappearance. The first may be called disappearance by extinction, the second disappearance by removal.

The hypothesis of disappearance by extinction is an appropriate name for (*B*3). The reason that led me to couple it with (*A*3) when

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[1] R. O. Kapp, *Science versus Materialism*, London, 1940, Chapter XXIV

[2] F. Hoyle, 'Stellar Evolution and the Expanding Universe', *Nature*, 1949, **163**, 196

[3] H. Bondi and T. Gold, 'The Steady State Theory of the Expanding Universe', *Monthly Notes Royal Astronomical Society*, 1948, **108**, 252

[4] W. H. McCrea, 'The Steady State Theory of the Expanding Universe', *Endeavour*, 1950, **9**, 3; 'Relativity Theory and the Creation of Matter', *Proceedings of the Royal Society*, 1951, **206**, 562

I first put it forward in 1940 and in subsequent contributions[1] was again the principle of economy of hypotheses. To the question for how long a given particle must exist the answer ' it may be for any time ' is less of a hypothesis than either the answer ' it must be for all time ' or ' it must be for the finite time that will elapse until the End of the World '.

On this view there is no need to assume that the rate of origin and the rate of disappearance are everywhere and at all times equal. For they are not assumed to influence each other. The *difference* between the two rates may be positive in one part of space-time and negative in another. The universe as a whole may be sometimes expanding and sometimes contracting, depending on whether the rate of origin for the whole happens to exceed or fall short of the rate of extinction. If one sets out to avoid any and every unsupported hypothesis one is left with the very barest minimum of assumptions. One must either abandon the subject or assume complete randomness. Anything more is based on faith and not on facts.

The hypothesis of disappearance by removal rejects (*B*3) and assumes that every particle that has once come into existence will continue for all time, as assumed in (*B*1), or at least until the End of the World, as assumed in (*B*2). The advocates of this hypothesis account for the observed finite density with which matter is spread over space in an ingenious way. They point out that, as matter originates, space must expand to accommodate it. And as matter is assumed to have been originating for an infinite time space is assumed to have acquired an infinite volume and to be expanding at an infinite rate. Thus disappearance is not regarded as extinction but as removal to distant regions.

It might be thought for a moment that the removed matter would continue to leave traces with an observer here and now, so that there would be an infinite accumulation of traces. But it would not be so. There is a limiting distance, beyond which objects in an expanding universe recede with a velocity greater than that of light. An observer can receive no evidence, even in theory, of the present or past existence of such objects. Their removal carries, one might say, all traces with it. There is something attractive, almost poetic, in the picture of heavenly bodies drifting on and on, eternally, into infinite space. Yet

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[1] R. O. Kapp, ' Effects of Origin of Matter on Cosmology ', *The Observatory*, 1948, **69**, 149 ; ' Development of the Universe ', *Nature*, 1948, **165**, 68 ; ' Theories about the Origin and Disappearance of Matter ', *The Observatory*, 1953, **73**, 113

I find myself unable to reconcile it with a rather important and fundamental scientific principle.

6 *The error of attaching physical meaning to infinity*

In mathematics infinity means just this: however large a number one cares to mention, one could mention a still larger one. The value of this larger one not being stated, the symbol for infinity does not stand for any specific value. Infinity is by intention and definition, an unspecified quantity. But how can a deliberately imprecise mathematical symbol apply to a physical reality? Surely it is axiomatic that there can be no infinite quantity of any physical reality. This helps to explain why, when in the general theory of relativity space was invested with physical properties, it was also found to be of finite extent. The same reasoning is implicit in the discovery in quantum mechanics that no physical quantity can be infinitely small. Are we now to go back on the insight that these discoveries gave us? And this is what (*B*1) does when it is coupled with (*A*3). It postulates a physical space with a rim, or edge, or boundary infinitely distant and receding from us at an infinite speed, at a speed moreover that increases exponentially during a finite time, no matter how short, by an infinite amount. When one sets foot upon the path represented by such a notion one cannot rest content with the may-be comforting concept of a simple infinity. One has to add infinity to function of infinity, over and over again, literally *ad infinitum*.

Even more disturbing, when one turns one's mind from a contemplation of mathematical abstractions to physical realities, is the notion of centres here and there around which, according to this hypothesis, matter must have been accumulating for all time. To point out that any such centres of accumulation must be in regions from which no trace can reach us is not to deny that their existence is implicit in the hypothesis. For it is assumed that centres of accumulation have begun at all times, some therefore in the infinitely remote past. Matter must have been falling on these for an infinitely long time; they must by now have grown infinitely massive; they must be imparting to all objects within their infinitely extensive, infinitely strong gravitational fields an infinite acceleration; they must be growing at an infinite rate; the gravitational forces at their centre must be infinitely intense; and so must the forces applied by gravitation at any finite distances from them however great these distances may be.

Such are some of the conclusions that one has to face when one follows the path that identifies all mathematical symbols with physical realities. Surely when one finds oneself on this slippery slope the wise thing is to retrace one's steps.

Let us remember the alternatives with which the two hypotheses about the manner of disappearance of matter confront us. Each obliges one, admittedly, to surrender something that one may like to believe. One must surrender either the axiom that an infinite amount of a physical quantity is a notion without meaning or the traditional belief in the permanence of matter and energy. But between an axiom based on logic and a belief based merely on tradition a scientist must not hesitate. Belief in the conservation laws is surely the more expendable one.

7 *A possible means of deciding between* ($B1$) *and* ($B3$) *by observation*

Both those who couple ($B1$) and those who couple ($B3$) with ($A3$) assume that new stars arise far out in interstellar space as the result of irregularities in the distribution of new matter. Mass densities slightly above the average are assumed to act as centres of gravitational fields and to grow by the capture of further new matter from their vicinity. But a moment's thought shows that the rate of growth cannot be the same for each hypothesis.

According to ($B1$) a new star must continue to grow without limit. But according to ($B3$) the rate of growth must become less as the star reaches a very large size and may at last cease altogether or even become negative. The reason is as follows.

To couple ($B3$) with ($A3$) is to assert that the rate of origin of new matter is approximately constant per unit volume while the rate of disappearance is approximately constant per unit mass. So a star would lose more matter the larger it was, and its replenishment rate by gravitational capture of new material would have also to increase with increasing size. Hence a growing star would depend on an ever increasing volume of space for new matter; the particles captured would be drawn, on the average, from greater distances; and so the probability that any one of them would reach the star before its disappearance by extinction would decrease as the star grew bigger. In other words, the replenishment rate according to ($B3$) could not be expected to keep pace with the loss rate; the latter would tend to gain on the former. Should they become equal the star would cease to grow. A consequence is that there must be an upper limit for the size of the star if ($B3$) is valid, but none if ($B1$) is.[1] ($B3$) thus predicts that a larger proportion of all stars must occur within a certain size-range than ($B1$) does. Data concerning the actual size distribution of all observable stars should therefore help towards a decision whether the hypothesis of disappearance by extinction or that of disappearance by removal is the more tenable.

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[1] R. O. Kapp, 'Development of the Universe', *Nature*, 1948, **165**, 687

VALUE-JUDGMENTS IN THE SOCIAL SCIENCES *

M. Roshwald

I

Social phenomena are classified today under several distinct branches of knowledge known as the ' social sciences '. Such a division is a comparatively recent introduction, as ever since the ancient Greeks a variety of social phenomena and problems had been dealt with together by what is today termed *political* philosophy. Quite often this philosophy encompassed problems which would be regarded today as legal, economic, educational, and sociological (this does not preclude the fact that jurisprudence became a distinct department of knowledge already in ancient Rome, and economics gained considerable independence in the eighteenth century). Indeed, the ancient dictum that man is a *zoon politicon* can be understood as attributing to man not merely a *political* nature, but also *social* qualities. The ancient polis, as is well known, having been not merely a political organisation, but also a community with specific religious and cultural characteristics, served as a unit of social relationship of manifold and rich contents. Though no clear and systematic distinction was attempted by the ancients between the different aspects of social life, this variety of aspects was included in the general approach, which thus was not narrowly political.

This undifferentiated treatment of what today seem heterogeneous subjects in the ancient Greek social-political philosophy was inherited, in a greater or less degree, by philosophers and theorists of later ages, though they might have composed a synthetic pattern of their own. Truly the dominant theme usually remained the political issue, but questions of religion were interwoven in the political philosophy of the Middle Ages, legal concepts pervaded the political thought of the social contract adherents, and the spirit of sociology entered the *l'Esprit des lois* of Montesquieu.

Moreover, this *mélange* of inquiry and speculation concerning social matters was permeated by an evaluational approach. Indeed the basic issue throughout the ages of political-social theory was the

* Received 17. ix. 54

good state, the *desirable* relation between the state and the individual, the *right* to rebel, and the like. The theorist and philosopher intended to find the true and final answer about the ethical problems of society and the State, no less, or even more, than to solve problems of a purely descriptive nature. Generally there was no systematic effort to distinguish between the factual and the evaluational side of political issues, facts having been used to strengthen norms, and norms having been expressed as facts (or perhaps even used to prove their existence). Typical in this respect is the famous doctrine of *natural law* (in the ethical-legal sense), implying that nature, i.e. *factual* existence, strengthens the authority of *moral* precepts. Conversely, *evaluation* of freedom seems to have induced Rousseau to state with assuredness that ' man *is* born free '. The classical example, perhaps, of confounding the factual with the desirable is to be found in Hegel's philosophy, though it would be difficult to decide whether the desirable determines his reality or reality moulds his ethical principles.

To be sure, one can find exceptions to this rule of mixing up descriptive and normative issues in the social-political philosophy of earlier times. Aristotle's *Constitution of Athens* or Machiavelli's *Prince* are quite typical examples of the descriptive approach to the subject-matter of politics. But attempts of this nature were comparatively negligible when seen against the background of the general ethical-political approach. The prevailing trend remained motivated by and imbued with the search for the ideal state and perfect society, differ as did the answers concerning the nature of the political *summum bonum*.

This traditional evaluational approach to social matters has been explicitly and emphatically opposed in the last decades by many social scientists and theorists in the name of scientific requirements and logical principles. It is difficult to state when this reaction started, but it can be broadly said that the nineteenth century, though on the one hand proceeding with the old tradition of ethical-social-political synthesis (though often along new tracks of philosophy of culture and society,[1] and using new concepts such as ' progress ' and ' evolution '), also witnessed the emergence of increasing departmentalisation of the social sciences and of their emancipation (or demand for emancipation) from value-judgments.[2]

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[1] Typical in this respect is, of course, Comte.

[2] The independence of social science from evaluative judgments is implied in Marx's ' scientific ' socialism (see further, section 2, iv). For an emphatic rejection of ethical evaluation Pareto's system is quite typical.

The purpose of this paper being a systematic analysis and not a historical tracing of the problem, let us proceed to the radical statement of the case for the independence of the social sciences from value-judgments, as formulated by some modern representatives of this trend. The contention will have to be analysed, in respect of its foundation and genesis (section 2) and logical validity (section 3).

2

An extreme exponent of the demand for ethical neutrality of social scientists, George Lundberg, maintains that the social sciences merely ' determine reliably the immediate and remote costs and consequences of alternate possible courses of action '.[1]

Though MacIver's stand on this problem seems less extreme, his following formulation should leave no doubt about his view concerning value-judgments in the social sciences :

> (*a*) Science is concerned not with the establishment of ultimate ends or values, but only with the relation between means and ends ; the ends can never be demonstrated, but only the relevance or adequacy of means to postulated ends.
>
> (*b*) Science is concerned with what *is*, not with what in the last resort *ought* to be . . .
>
> (*c*) Social science has *as part of its subject matter* the valuations operative in social institutions and organisations, but not the valuations of these valuations on the part of those who investigate them.[2]

The above statement is quite explicit and clear, and represents a widely accepted (though not universal) opinion among social scientists in various branches. It should be noted that the demand for the ethical neutrality of the social sciences is closely intertwined with the notion that ultimate values are subjective or relative and do not allow of an objectively valid analysis and foundations. That values can never be demonstrated is a widely accepted belief not solely among social scientists. Let us examine the probable foundations of these views, viz. the demand for ethical neutrality in respect of the social sciences, and (its main foundation) scepticism concerning the possibility of universal ethics. Though not all of the following reasons will serve as an explanation in each case of such an attitude, some of them will be helpful in the understanding of most cases.

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[1] George Lundberg, *Can Science Save Us?* p. 29 (quoted from George Simpson, *Science as Morality*, Yellow Springs, Ohio, 1953, p. 10)

[2] MacIver and Page, *Society*, London, 1950, p. 618

(i) *Division within the social sciences*

At the beginning of this essay reference was made to the historical fact that out of a relatively unified social-political branch of knowledge, prevailing throughout the ages, a variety of specialised sciences started to emerge during the last few decades. Such a division was bound up with attempts at delimitation of the various social sciences, and it may be conjectured that the general tendency to divide, separate, and assign boundaries influenced the tendency to regard evaluation as belonging to a specific branch of inquiry too, and therefore as being outside the province of the various social sciences. It seems by no means incidental that the demand for purely descriptive social sciences has been far more typical of the United States, where differentiation and professionalisation in this field is far ahead of any other place, than of Great Britain, where the establishment of new independent social sciences in universities has met with considerable opposition. Such an attitude did not necessarily need to imply ethical *relativism*, but it meant ethical *neutrality* on the part of the political scientist, the anthropologist, and the social psychologist (not to mention the business or public administrator), in the way that they remained neutral in respect of, say, engineering or medicine.

Possibly the independence of the social sciences from ethics became stressed even more than that, as ethics was deemed to belong to the domain of philosophical, speculative thinking, while the social sciences intended to establish themselves as practical sciences on the pattern of engineering and medicine.

(ii) *Qualitative difference between means and ends*

The contention of many social scientists, however, is not merely that ethical judgments lie *outside* their specific fields of inquiry, but that no field of ethics proper *exists*, which can be systematically investigated with a view to attaining a generally valid truth. This is explicitly stated in the above quotation from MacIver's and Page's *Society*, a clear demarcation line being drawn between ' ends and values ' on the one hand and ' the relation between means and ends ' on the other hand.[1] Only the latter sphere is deemed as suitable for

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[1] Cf. with the following distinctions made by Max Weber :

' At the same time, our Chinese can lack a " sense " for our ethical imperative and he can and certainly often will deny the ideal itself and the concrete value-judgments scientific inquiry, while the former, *in principle*, remains outside logical investigation.

The idea behind such a formulation seems to be that the two spheres are epistemologically different—the one (of means) allowing inquiry, the other (of ends or values) confined to some sort of an arbitrary decision on the part of the attitude-forming person.[1] It is the *material* of the two spheres that differs, and our mental approach should be adapted to its subject-matter. The scientific-logical procedure does not suit the subject-matter of *true* valuation (though it may suit the subject-matter of the psychological and sociological phenomenon of valuation), and, as scientific method seems the only way to obtain true statements, there seems to be no escape from the conclusion that value-judgments cannot be true in the sense that descriptive statements can be.

(iii) *Historicism*

The development of historical inquiry and of the philosophy of history during the nineteenth century has exercised a profound influence on the attitude to value-judgments. The historian tends to regard things not *sub specie aeternitatis*, but *sub specie temporis*. If the historical method is employed in respect of social philosophy, the fundamental question asked is not any more whether a philosophy is true or false, but in which way it is conditioned and moulded by the circumstances of a certain time.

It would be easy to quote innumerable instances of historical explanations of value-permeated outlooks and philosophies, explanations

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derived from it.' (' " Objectivity " in Social Sciences ', published in *The Methodology of the Social Sciences*, Glencoe, 1949, pp. 58-59.)

' Even such simple questions as the extent to which an end should sanction unavoidable means, or the extent to which undesired repercussions should be taken into consideration, or how conflicts between several concretely conflicting ends are to be arbitrated, are entirely matters of choice or compromise. There is no (rational or empirical) scientific procedure of any kind whatsoever which can provide us with a decision here. The social sciences, which are strictly empirical sciences, are the least fitted to presume to save the individual the difficulty of making a choice, and they should therefore not create the impression that they can do so.' (' The Meaning of " Ethical Neutrality " ', op. cit. pp. 18-19)

[1] Weber quite rightly stresses the ' belief in validity ' as an essential characteristic of evaluation ; but this belief remains in his opinion a *subjective* matter : ' Only on the assumption of belief in the validity of values is the attempt to espouse value-judgments meaningful. However, to *judge* the *validity* of such values is a matter of *faith*.' (' " Objectivity " in Social Science ', op. cit., p. 55)

implying the relativism of such philosophies. The following quotation, picked out at random, seems quite typical: '. . . in England individual rights became an adequate philosophical support for commercial and industrial expansion under a policy of *laissez faire*.'[1] Thus the main question seems to be whether the individual rights harmonise with the economic factors of a certain place and time and not whether they are justifiable *irrespective* of time and place. The problem of whether a social philosophy is true or not, and *ipso facto* whether the values propounded in it are true or false, seems to become irrelevant.

Such an approach is often the outcome of Marxist fashion which prevails even among many non-Marxist scholars. It is no accident that in the above quotation the basic historic factor is economic. But this need not necessarily be so, and a historical approach regarding, say, the quest for power as the basic driving force in history could similarly conceive of moral ideals as a function of the political condition of time and place. The resulting opinion of the relativism of social moral philosophies would be similar in principle.

(iv) *Prognostic claims*

Social scientists only too often claimed ability to prognosticate. Already Polybius spoke of 'the regular cycle of constitutional revolutions, and the natural order in which constitutions change, are transformed, and return again to their original stage'.[2] But the claim of prognostic validity of sociological laws became most manifest with the theory of Marx.

The very concept of *scientific* socialism was meant to signify that Marx's theory was not just a commendable suggestion for people of good will, but a scientific discovery concerning human relations, in the sense that Newton's laws formed a discovery in respect of natural phenomena[3] (in the realistic sense that physical laws are not merely

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[1] Quoted from Sabine, *A History of Political Theory*, London 1951, p. 545

[2] Book VI. English translation by Evelyn S. Schuckburgh, 1889. Quoted from Coker, *Readings in Potitical Philosophy*, New York, 1938, p. 120.

[3] Cf. Engels's estimation of Marx's work: 'Just as Darwin discovered the law of evolution in organic nature, so Marx discovered the law of evolution of human history.' (Quoted from the article on 'Socialism' in *Enc. of the Social Sciences*, Vol. XIV, p. 198a).

That such a deterministic outlook did not preclude a dynamic advocacy of proletarian revolution is a major paradox of Marxian system, which does not allow, in the present writer's opinion, of a *logical* solution and must remain a contradiction. Despite that, *psychologically* the necessity of the 'final victory' moved the adherents a working-hypothesis, but a discovery of objective relations in the external world). Such a ‘ physical ’ conception of social relations was in its very nature deterministic, and as such it tended to collide with an ethical approach, which is mostly, though not necessarily,[1] indeterministic. Even though it cannot be alleged that Marxism is free of value-judgments, the deterministic element in the Marxian approach implies the practical futility of discussion about the best way to be chosen by men in forming their social relations. Thus ethical discussion and ethics, practically remaining outside the scope of the ‘ scientific ’-deterministic approach to social phenomena, have also been dispensed with theoretically.

Truly, Marxian determinism seemed concordant with the value-impregnated concept of progress, as the final withering away of the State (for example) seemed to resemble the eternal bliss, but still there seemed no place for ethical *discussion,* as the good outcome seemed natural and *necessary*. This seems true also of the theories of social evolution of Spencer[2] and many others. The deterministic theory of society, though allowing for an ethical conception of the sociological change, does *not*, if consistent, allow discussion of the morally desirable (except as a playful activity, like chess). The theorists themselves were not always consistent in this respect, but the consistent conclusion

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of the creed to participate in its realisation and served a powerful source of revolutionary activity.

The inventor of this paradoxical activating determinism is Marx's master (in this respect), Hegel, whose following words are typical ; ‘ The nation to which is ascribed a moment of the Idea in the form of a natural principle is entrusted with giving complete effect to it in the advance of the self-developing self-consciousness of the world mind. This nation is dominant in world history during this one epoch and it is only once that it can make its hour strike.’ (Hegel's *Philosophy of Right*, translated by T. M. Knox, Oxford, 1942, pp. 217-218.) It does not seem that the ‘ chosen ’ nation may dispense with the honour of embodying the world mind, but still the pointing to the moving clock was a powerful psychological means to spur the Germans to fulfil their mission in world domination.

[1] One may develop an ethical system, despite holding a deterministic outlook, and evaluate the things existing and unchangeable. Thus, for example, we may regard death as undesirable, even though we cannot abolish it. An ethics, however, combined with a strictly deterministic philosophy, would have no practical significance as a motivating force.

[2] ‘ The ultimate development of the ideal man is logically certain—as certain as any conclusion in which we place the most implicit faith.’ (*The Principles of Sociology*, Vol. III : quoted from M. Ginsberg, *The Idea of Progress*, London, 1953, p. 4)

of eliminating ethical discussion from social inquiry seems to have influenced many a social scientist who otherwise often disagreed with the deterministic progressivists.

(v) *Quantitative approach*

The attempt to conceive of social phenomena as following the deterministic pattern of some discoverable laws was, in a degree, influenced by the methodological approach of the natural sciences. The pattern of the natural sciences influenced even more directly some more recent tendencies in the social sciences, thereby strengthening the disregard for evaluational distinctions.

If a methodological ideal of the natural sciences is to express the variety of its data in *quantitative* terms, then this method has been applied with success also in respect of many manifestations of social life. Indeed, statistics has been growing in importance as a tool for advancing sociological knowledge. This development has evoked a tendency to make quantitative symbols the *sole* means of expressing sociological knowledge. This tendency is vigorously put forward by Lundberg :

> Geometric, arithmetic, and algebraic ways of expressing relationships usually come with the maturity of every science . . . The more intricate and variable is the situation we wish to describe the more dependent we become upon mathematical systems of symbolisation. Sociology has for some time felt this need . . .. More recently much work has been done in the development of measuring instruments adapted to societal phenomena hitherto unmetricised. With the rapid developments in this field, the time is ripe for the systematisation of the whole field of general sociology in quantitative symbols which, however unique the subject-matter to which they refer and however unmetricised as yet some of these phenomena may be, can nevertheless be manipulated according to the already established and tested rules of mathematics.[1]

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[1] G. Lundberg, *Foundations of Sociology*, New York, 1939, pp. 122-123

It is noteworthy that a quantitative approach for classification of cultures is applied by a modern anthropologist. Though this is a comparatively simple use of quantitative technique, the case is quite typical : ' In my opinion one profitable way . . . is to study *quantitative* differences between cultures, a far different thing from the pursuit of value judgments . . . I also believe that the study of the quantitative method is profitable because it is the method of systematic science which has proved fruitful in all other fields of learning in which it has been used.' (Carleton S. Coon, *A Reader in General Anthropology*, London, 1950, p. vii.)

Such an exclusively quantitative technique, though it implies the hope of encompassing ‘ some as yet unmetricised phenomena ’, only too naturally tends to designate as non-scientific whatever seems beyond hope of expression in mathematical symbols. This in fact results in discarding value-judgments, which are deemed as non-quantifiable,[1] from the realm of the scientific approach.[2]

(vi) *Experimentalism and behaviourism*

Natural Sciences deal with non-voluntary, material phenomena. Any notions of motivation or volition in respect of them are either ancient animism or modern superstitions. This approach of the natural sciences has found its adherents also among students of man, the behaviouristic school in psychology being a typical example. The tendency found its way also into the social sciences, and Lundberg should be quoted again as a clear exponent of this view :

> We no longer find it helpful to bother with the motives of tornadoes, bacteria, or even of the higher animals, except man. . . . In the social sciences it is felt we must go ‘ farther ’. . . . It does not follow that we should engage in a futile search in nature for entities to correspond to these animistic words that happen to be part of our vocabulary. When the interaction of the observable components of a situation has been described scientifically, purpose and motive have also been described, and all scientific ‘ purposes ’ have been served.[3]

If there is no room in scientific presentation for purpose and motivation, surely there is no place for ethical evaluation. If so, ethical value-judgments are not only illegitimate criteria of the social scientist’s approach, but even superfluous as objects of the sociologist’s research, unless stated in behaviouristic terms. It is this extremity which emphatically shows the tendency (influenced by the natural sciences) to discard as unscientific whatever does not fall within the category of observable (and possibly experimental) facts. As value-judgments seem outside this category, they are deemed unscientific. *A fortiori,* they cannot be used as *tools* of the scientist’s approach, as an ingredient in his methodology.

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[1] It should be pointed out that a mathematical-logical approach has been attempted even in respect of valuation, though this is of course a rather exceptional trend. See, for example, Theodor Lessing, *Studien zur Wertaxiomatik: Untersuchungen über Reine Ethik und Reines Recht*, Leipzig, 1914.

[2] G. Lundberg, op. cit.

[3] Lundberg, *Foundations of Sociology*, p. 222

(vii) *Psycho-analytic theory*

If the above tendency is typical for a physicalistic approach to human sciences, it is noteworthy that the anti-evaluational trend has been strengthened also from an opposite direction, namely, a school in modern psychology which assumed the specifically psychological nature of mental phenomena. In the psycho-analytic theory motives for action are in a way more important than the action itself, and in this respect at least it is diametrically opposed to behaviourism. Nevertheless, the approach of psycho-analysis to ethical motivation is highly sceptical. Conscience is not regarded as an original manifestation of human nature, but solely as a derivative of certain emotional factors of childhood combined with the internalisation of external authority (usually of the father and mother).[1]

Such an explanation in fact explains away the moral faculty of human beings. Moral distinctions become a function of psychological and environmental factors, and their claim to validity as norms of behaviour turns out to be a mere *psychological* phenomenon, not different in kind from an Oedipus complex. To be sure, the psycho-analyst, in fact, uses certain norms when he determines that somebody *should* be given a psycho-analytic treatment, but this fact has been far less stressed than the relativistic conception of moral distinctions by this school, with the result that another powerful factor in modern thought worked for the discarding of value-judgments from the realm of human sciences.

(viii) *Anthropological research*

Social anthropology can boast of an immense increase in detailed knowledge about the ways of living and cultures of a variety of primitive societies throughout the world. Such inductive accumulation of knowledge has shown how deep run the differences between cultural patterns and the outlooks, beliefs, and norms of various communities. It suffices to have a look at such a study of a number of different societies as Ruth Benedict's *Patterns of Culture* in order to see the variations in current human ' philosophies '. This cultural diversity is understood by anthropologists as ' cultural relativity ', and the conclusion is drawn that we have to accept all cultural forms as 'equally valid patterns of life'.[1]

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[1] This is, necessarily, a very brief account of the psycho-analytic conception of conscience. For a systematic elucidation of this conception, the reader may be referred to J. C. Flugel, *Man, Morals and Society*, London, 1945, Chapter III. Of Freud's own writings, among others, *The Ego and the Id* (1923) should be consulted.

The ethical relativism implied in such an approach is obvious. It is no wonder, therefore, if the Executive Board of the American Anthropological Association formulated the following statement on Human Rights to the respective commission of the United Nations:

> Respect for differences between cultures is validated by the scientific fact that no technique of qualitatively evaluating cultures has been discovered.[2]

This stand is quite typical for ethical neutrality of many a social scientist, though the statement should be reversed in order to reveal the genetic relation between modern anthropology and scientific amoralism. Such a reversed statement would run as follows: 'The lack of respect for qualitative evaluating of cultures is validated by the scientific fact of differences between cultures.'

There is also perhaps an additional reason, of a psychological nature, for refraining from an evaluational approach to the study of cultures. The researcher, in order to grasp the meaning of a primitive culture, has to learn a foreign (and entirely strange) language, to live among his objects of inquiry, to change his pattern of life, or at least greatly to modify it. The success of a field anthropologist seems conditioned by his capacity to identify himself personally with the community which he is studying. Such an identification, though always limited, carries, if not acceptance, then at least tolerance, of the moral norms of the community being investigated. That to understand is to forgive need not be a *logically* valid statement, but it reveals a rather common psychological phenomenon. Thus the anthropologist's attempt at a profound comprehension of a primitive culture leads to a forgivingness which results in tolerance in respect of varying norms and in ethical relativism. Here is another source of the modern tendency, which is not restricted to anthropologists alone.

(ix) *Variety of ethical systems*

Not only is the variety of cultures, which in practice seems to mean variety of ethical evaluation, a source of the ethical relativism of social scientists. Another reason rooted in history is the variety of *theoretical* ethical systems. No doubt there is a great difference, to quote some examples, between an Epicurean and a Stoic, a Utilitarian and a Kantian.

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[1] Ruth Benedict, *Patterns of Culture* (1934). Quoted from Mentor Books edition, 1949, p. 257.

[2] Quoted from Carleton S. Coon, op. cit., p. vii.

To an external spectator at least such variance of opinion concerning the fundamentals of ethical evaluation between admittedly outstanding personalities and intellects makes it quite obvious that the hope of an objectively-valid foundation for moral evaluation is futile, and has to be discarded by any scientifically minded person. A typical exponent of this trend of thought is Pareto, who, as Ginsberg puts it, 'makes much of the argument that if ethical judgments permitted of rational examination, ethics would have made greater progress than it appears to have made since the days of Aristotle '.[1]

(x) *Democratic procedure*

Another possible factor making for disbelief in universal ethical validity can be sought in democratic procedure. This may seem strange at first sight : Democracy is deemed by democrats to be the *good* form of government, and this evaluation is made explicitly, its general validity being implied. Though this point is admitted, it seems to us that certain aspects of democracy exercise an influence in the opposite direction, viz. that of ethical scepticism.

Democracy works on the practical rule that the *majority* decides on the course of action to be taken in any specific issue. Such a majority usually is not fixed permanently, and consequently contradictory decisions are often made on similar issues. If, despite such instability, democracy is regarded as a *good* form of government, some people tend to draw the conclusion that a good government is such where the *right* (= good) action is determined arbitrarily by an incidental majority. This amounts to saying that *good* government (= democracy) is such which regards (moral) *good* as relative.

Such a stand is obviously self-contradictory, for ethical validity and scepticism are assumed here simultaneously, but such a contradiction does not prevent this view from exercising social influence. And a *partial* influence of this conception is that social issues cannot be decided by generally valid moral criteria. Here again we have moral relativism or subjectivism in respect of social phenomena, which could have found its way into the social sciences.

Though the (alleged) logical reasons, as well as genetic antecedents, for ethical relativism may be numerous, it would be erroneous to conclude that value-judgments, whether explicit or implicit, are absent in the writings of social scientists adhering to this view. Quite the opposite is often the case.

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[1] Morris Ginsberg, *Reason and Unreason in Society*, 1947, Chapter IV, 'The Sociology of Pareto', p. 89

Marx's ' scientific ' determinism does not prevent his advocating that working men of all countries should unite (a demand implying evaluation). It has already been mentioned that the Freudian psychologistic approach to the phenomenon of conscience, is not followed consistently by a psycho-analyst, who *de facto* strives to cure his patient ; should he attempt to make him more sick, it is quite improbable that the lack of conscientious restraint in such case would merely be regarded by other members of the school as freedom from super-ego domination.

That anthropologists become entangled in a similar contradiction is obvious from the fact that their supposition of cultural (and ethical) relativism induces them to demand ' respect for differences between cultures ' (to use the already quoted formulation of the American Anthropological Association). Such ' respect ' is obviously a moral attitude. (It could be argued on the anthropologists' premisses that even such a demand for universal toleration of cultures is but a specific trait of the unique culture of the American Anthropological Association, and need not be tolerated more than an equally valid, or equally invalid, demand of a fanatical religious sect of a peculiar culture of its own, to enforce its creed on everybody else, the kindly anthropologist included. Should, however, intolerance be tolerated, there will be no tolerance for tolerance, and the demand for ' respect ' of cultures could not be maintained.)[1]

The above examples show that it is far more difficult for social scientists to discard value-judgments, in practice, than to proclaim it as a scientific creed. This, however, does not prove that moral relativism and the demand for ethical neutrality in the social sciences are erroneous, for (to use MacIver's and Page's neat formulation) ' one cannot . . . conclude that a principle is unsound . . . because its proponents fail to live up to it '.[2] Marx might have refrained from advocating a desirable action. Freud could have stopped treatment of patients, and the American Anthropological Association could pursue its inquiries without advocating tolerance of cultures. No contradiction would be involved in such a behaviour, and the demand for ethical neutrality in the social sciences would remain unimpaired.

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[1] Some additional instances of sociologists (von Wiese and Pareto) failing to live up to their own demand of neutrality in respect of value-judgments are quoted by MacIver and Page, op. cit., p. 617. Cf. also Robert S. Lynd, *Knowledge for What?*, Princeton University Press, 1945, pp. 184-185.

[2] MacIver and Page, op. cit., p. 617.

We should examine, therefore, the (above enumerated) positive reasons put forward in favour of ethical neutrality, and not merely point out the inconsistencies in practice of some advocates of such an approach. This will be done in the coming section with reference to each of the above ten points.

3

(i) *Division of the social sciences and its relation to value-judgments*

To infer that scientific concentration on a narrow subject-matter frees the scientist from value-judgment seems to us fallacious. Truly, scientific procedure requires delimitation of sciences from each other, but such a delimitation does not mean *independence* of each science. Even though a physician deals with matters differing from the chemist's subject-matter, medicine is by no means regarded as independent of chemistry. To infer from the *division* of sciences the *autonomy* of each is fallacious. Thus, self-enclosure of each social science cannot be regarded as a justified reason for its independence of a distinct branch of knowledge, namely, ethics. Economics, political science, sociology, education, etc.—though each possesses a peculiar sphere of inquiry—may nevertheless be dependent on value-judgments, at least in the sense that medicine as an applied science is ultimately based on the value-judgment that the health of the individual is desirable.

This dependence is in no way obstructed by the fact that ethics is ' speculative ' while the social sciences are ' practical '. For speculative elements may remain a tool of a practical inquirer ; indeed the methodological aspect of any practical science (and there can be no science without a methodology!) cannot be analysed without speculative, philosophical thinking.

(ii) *The bearing of the difference between means and ends on the possibility of valid value-judgments*

It should be pointed out that the identification of values with ends and of factual connections with means is a simplification of the issue. Ethical analysis based, say, on a hedonistic assumption of happiness as the *summum bonum* may lead into intricate discussions as to how to attain that end. The problem of human virtues, falling within the discussion of ethics, will be in the case of a hedonistic system a problem in adapting means (virtues) to the accepted end (happiness). Similarly, the attempt to subordinate liberty and equality to the supreme principle of happiness will deal with relations of means to an end. The distinction, therefore, between means and ends is by no means identical with the distinction between descriptive and normative fields, and consequently the discarding of ethics as non-scientific on account of such illusory identification is unfounded.

Still, it could be maintained (and rightly so) that a qualitative difference between the sphere of the descriptive and the normative exists. In the former sphere statements are joined by the copula ' is ', in the latter by ' ought to '. This difference may be deemed a ground for the allegedly non-scientific character of valuations. In our opinion such difference should not mean that valuations do not allow of an inquiry leading to valid propositions. It can be pointed out that such an inquiry is quite difficult and that moral issues seem hard to ascertain. But this is also the case with many problems within the descriptive realm. Let us examine an example.

The social scientist may investigate the most efficient way to attain a high level of general prosperity in a given community. To decide whether a ' free ' economy or a ' welfare ' state *is* a more efficient way for that purpose is by no means a simple task, for the complexity of economic conditions, psychology of workers, cultural tradition, public administration, and so forth have to be taken into consideration. We agree that to decide whether ' equality ' or ' liberty ' *should* be preferred may not be a simpler issue, but neither does it seem more complicated ! There is no reason why the former, descriptive problem should claim to be more susceptible of rational inquiry than the latter, normative one. On the other hand, a normative proposition that ' general welfare is a desirable end ' may be regarded as of general validity no less than the descriptive statement that ' work is one of the conditions for prospering community '.

These are only examples pointing to the conclusion that the epistemological distinction between descriptive and normative judgments has been exaggerated, and that the latter need not be discarded because of this distinction.

(iii) *Historicism and social-moral philosophy*

We do not intend to belittle the importance of historical method in general or its use for the inquiry of social philosophies. The achievements due to this approach may have been illustrious.

But it would be false to imply that the historical method is the only—or even the main—clue for the understanding of ideas, moral ideals included. It would be easy to show by specially collected examples that there are ideas and ideals that cannot be plausibly explained in terms of economic forces or political factors, and have to be attributed to the independent and spontaneous creativeness of certain outstanding individuals.

And quite independently of the issue whether such individuals existed or not, it can be argued that the question whether a certain ideal is true or false is at least as legitimate and as significant as the question concerned with the factors that possibly gave rise to the ideal.

We do not intend to discard altogether the history of social philosophy or the historical approach to the examination of any such a philosophy. These may be useful by explaining many issues, which would otherwise remain obscure (mainly, perhaps, by explaining the *mistakes* of great philosophers). But they surely do not eliminate the necessity and validity of another, logical, approach which asks whether the tenets of a social philosophy are sound or unfounded, and purports to arrive at a *valid* social-moral philosophy in a non-historical sense.[1]

(iv) *Prognosis and the social sciences*

It has already been remarked that a deterministic view of social change, implied in the prognostic claims of certain theorists, need not preclude evaluation of social occurrences, though such evaluation would be of no practical significance.[2] But it can also be justly argued that the very assumption of determinism in social phenomena is unfounded. To be sure, it does not seem possible to *disprove* social determinism in the way that a false statement in mathematics may be disproved. For should we show cases of people or societies behaving in a way contradicting the deterministic pattern of a prognosticating theorist, it will be always possible to maintain that not all the determining factors are known so far, but nevertheless human behaviour is precisely determined by some such factors. Though no conclusive answer can be given to such argumentation on the metaphysical level (in the sense of deciding the ultimate truth about this matter), there are epistemological grounds for rejecting social determinism in its extreme form.

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[1] The discussion in the above passage owes much to the stimulation of a lecture by Professor Leo Strauss of the University of Chicago on 'What is Political Philosophy', who should not, however, be held responsible for the views expounded here.

[2] See p. 192, note [1].

The first ground is that as long as there are many phenomena which can be neither prognosticated nor incorporated in uniform deterministic laws, it is more convenient not to accept the deterministic hypothesis. It seems that many manifestations related to man and society cannot be explained by any deterministic hypothesis.[1] Believers in progress can be shown regressions, psycho-analysts can be shown people acting rationally despite some psychological complexes, and materialists may meet behaviour unexplainable by material elements. As long as this is so, there is no reason to assume an extremely deterministic hypothesis in respect of social phenomena.

Another point of epistemological criticism is that the supposition of social determinism by various theorists is not the result of observation of data, but (to a great extent at least) an *a priori* postulate. Marx's ' dialectical materialism ' is influenced by the deterministic pattern of the ' dialectic ' of Hegel, which is far from being founded solely on inductive observation. That such rather arbitrary assumptions (though claiming logical validity) are a poor reason for accepting social determinism need not be enlarged upon.

To be sure, deterministic approaches to the social sciences are not founded merely on certain obscure philosophies, but also on the pattern of the natural sciences. Indeed the claim to be *scientific* seems to many to imply the possibility of revealing deterministic laws rendering prediction possible. The moment that knowledge of society claimed to be scientific, it seemed obvious to many that the deterministic outlook had to be adopted. Our opinion is that whether ' science ' has to be deterministic or not is a purely verbal issue : define ' science ' as you like, and accordingly call social knowledge ' scientific ' or not. The real issue is whether social *phenomena* allow of a deterministic approach or not. Our knowledge so far points to the fact that any explanation of social (and human) behaviour is deterministic only to a certain extent, while a considerable part of such behaviour eludes any formal conformity. In this respect social science (or ' knowledge ', if you prefer the term) seems as radically different from natural science as man is from inanimate objects.[1]

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[1] A clear and interesting examination of the problem, pointing partly at least to indeterministic conclusions, is to be found in J. S. Mill, *Representative Government*, Chapter I.

If social behaviour remains to some extent beyond the reach of deterministic laws, there remains a domain which seems to be open to the influence of value-judgments. Such judgments need not, therefore, be discarded by social researchers on the ground that, society moving according to predestinated laws, evaluation has no practical significance.

(v) *Quantification and evaluation*

To discard value-judgments from scientific methodology on the ground that quantitative technique is an essential characteristic of scientific procedure is open to the same criticism as that which may be levelled against the extreme adherents of determinism. The technique of scientific inquiry should not be postulated wholly *a priori*, but has to be adapted to the subject-matter of inquiry. If there are social phenomena which allow quantitative technique, such technique may be beneficially used. But to discard any subject (such as value-judgments) from systematic inquiry on the ground that it cannot be measured resembles cutting feet in order that they may be adjusted to the size of available shoes; or, to borrow a direct statement that expresses the point, it is ' to cut the problem down to the size of the techniques, instead of lifting the techniques up to the size of the problem '.[2]

(vi) *Physicalism and ethics*

The behaviouristic tendency in the social sciences is but a translation into the realm of human studies of a methodological principle of physics and the natural sciences, which requires the construction of knowledge ultimately on sense-perceptions. Our criticism of this aspect of the impact of the methodology of the natural sciences on social knowledge resembles our criticism of the previous aspect. Though we cannot *see* an ethical evaluation itself (only its results), nor touch it, or even smell it, it seems to us fallacious to treat it as non-existing within the realm of systematic inquiries.

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[1] It is worthwhile to quote in this connection Hume : 'When I am at a loss to know the effects of one body upon another in any situation, I need only put them in that situation, and observe what results from it. But should I endeavour to clear up after the same manner any doubt in moral philosophy, by placing myself in the same case with that which I consider, it is evident this reflection and premeditation would so disturb the operation of my natural principles, as must render it impossible to form any just conclusion from the phenomenon.' (*A Treatise of Human Nature*, Introduction. Quoted from Everyman's Library edition, p. 7.)

[2] George Simpson, *Science as Morality*, p. 30

To state the case positively, we cannot but refer to the distinction of some such classical philosophers, as Locke, who though stressing experience as the only source of the ' materials ' of knowledge, divided it into external and *internal* experience (or ' sensation ' and ' reflection ').[1] Evidently, evaluational distinctions are within the domain of the internal experience. The reality of moral judgment is consciously *experienced* by us, and should not be discarded, as the experience of the roundness of a table is not discarded by us. The point is simply that physicalism cannot express everything—not moral experience at least. Nevertheless, such experience is genuine, and may possibly be of importance in respect of the methodology of the social sciences.[2]

(vii) *Psycho-analytical theory and moral distinction*

Whether psycho-analytic explanations of the phenomenon of conscience are correct or not cannot be examined without a profound analysis of psycho-analytic theory, which cannot be attempted here. A possible general objection is that the analysis of neurotic patients, whatever its results, should not be generalised and applied to humanity as a whole. Though psycho-analysis also examines literary works and primitive cultures in its own peculiar way, its basic methodology is founded on therapeutic techniques and on findings connected with abnormal phenomena of human beings. The *generalisation* of such findings may serve as an invitation to criticism.

Besides this hint at a general criticism of psycho-analysis, our point should be reinforced by a direct objection to the psycho-analytic explanation of conscience. It seems to us that what Freud succeeded in proving was merely the psychological-environmental conditioning of the *contents* of conscience in many cases, but not the absence of an essential impulse in our consciousness to behave according to what seems to us to be morally desirable. The evidence given by Freud and his disciples in favour of the psycho-analytic theory of the genesis of conscience does not seem to us sufficient to abolish what is experienced by us as a moral impulse claiming validity (as clearly distinct from a mere desire). It seems to us that there remains an essential difference between emotion and desire as psychological phenomena (in the narrow sense of the term) on the one hand, and the evaluative and moral faculty on the other hand ; the latter seems to form a mental manifestation *sui generis* to be classed alongside such a phenomenon of the human mind as logical thinking. Psycho-analysis (or any school of psychiatry) may show that many illusive ideas concerning descriptive matters formed by a maniac are a mere outcome of some emotional distortion, but that would not abolish the logical validity of thinking *in general.* Similarly, psychological explanations of some manifestations of conscience in a variety of cases does not altogether annihilate the existence of conscience as a primary human element, and still less prove the impossibility of a generally valid moral judgment.[1]

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[1] See John Locke, *An Essay concerning Human Understanding*, Book II, Chapter I, sections 1-5.

[2] It is interesting in this connection to note the following attitude of Max Weber : ' The task of an ethically neutral science in the analysis of syndicalism is completed when it has reduced the syndicalistic standpoint to its most rational and internally consistent form and has empirically investigated the pre-conditions for its existence and its practical consequences. Whether one should or should not be a syndicalist can never be proved without reference to very definite metaphysical premises which are never demonstrable by science.' (' The Meaning of " Ethical Neutrality " ' in op. cit., pp. 24-25.)

Value-judgments are regarded as derivable from ' metaphysical premises ' only, and therefore outside scientific domain, which deals with (*a*) rational structure and (*b*) empirical (external) phenomena. The possibility of generally valid ethics based on *empirical internal* phenomena is not even mentioned here (quite typically for Kantian Germany).

In fact, as we have already seen,[2] psycho-analysis cannot dispense with value-judgments any more than it can dispense with logical reasoning. Its content is constructed with the help of the latter, while its practice is conditioned by the acceptance and use of the former.

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[1] For a more elaborate and many-sided examination of the bearing of psycho-analysis on ethics, the reader is referred to Professor Morris Ginsberg's article on ' Psycho-Analysis and Ethics ', published in the *British Journal of Sociology*, 1952.

[2] The practical need of evaluation in psycho-analysis *qua* a therapeutic system has been mentioned at the end of section 2.

A typical illustration of the ' ambivalent ' attitude of psycho-analysts to value-judgments is manifest from the following quotation : ' The propensity to cut and shed blood, for example, may appear beautifully disguised in the surgeon (sublimation), less prettily so in the butcher (still a sublimation), and unhappily so in the neurotic who feels impelled to cut someone (symptom) ; and dangerously so in the Jack the Rippers who carry out such impulsions.' (Karl A. Menninger, *The Human Mind*, New York, 1945, the 1949 printing, p. 280.)

On purely psychological premisses, there should be no difference between shedding-blood and surgery (if any practice should be preferred on psychological grounds, it would be the former one, being a direct expression of emotions). If, nevertheless, psycho-analysts accept the distinction between ' beautiful ' and ' dangerous ', as they (happily) seem to, they accord *objectively valid* status to the attitude of their super-ego, in contradiction to their professed creed.

(viii) *Anthropology and universal morality*

The anthropological argument against universal morality seems to us entirely fallacious. How far the moral outlooks of different societies differ, and whether a common moral denominator may not be found for the entire humanity (with some obvious exceptions, such as maniacs and Hitlers) is a matter which does not seem definitely concluded, despite the significant differences in the value-judgments inherent in diverse cultures.[1]

But even if it were shown that, ultimately, various cultures irreconcilably contradict each other in their value-judgments, this would not prove the relativist's case. The reason is quite simple: various primitive peoples hold entirely different views about the nature of the universe, about human physiology, and other descriptive matters; does this induce anybody to conclude that the physicist's or the physiologist's opinions are merely relative and of no objectively valid significance? Of course, this conclusion is not drawn by any anthropologist, and the status of primitive physics is not regarded as equal to that of modern physics. There seems no logical reason, in this respect, to treat morals differently to physics. As an anthropologist inquires into the belief of totemic peoples concerning their kinship with a totem-animal, regarding it as fictitious and *false,* so he may inquire into the moral views of head-hunters without rejecting his view that head-hunting is truly *bad.*

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[1] See, for example, the following: 'We may approach this problem by glancing briefly at what may be learnt from comparative ethics of the extent of variance and stability in moral codes. It may safely be said that comparative ethics knows of no pre-moral stage, that is, of any societies where there are no rules, upheld by the general judgment of the people, distinguishing between what may or may not be done. If there has been a development of morals, it has been a development within the field of morality and not a development of morals out of something else. When we look at the codes in detail we find that all known societies insist on some degree of internal peace and order and tend to give public approval to whatever acts contribute to the satisfaction of social needs and the preservation of the continuity of the group, and similarly to disapprove of whatever acts disturb social relations and social peace. Contrary to widespread views on this matter we find, amidst noteworthy divergencies, striking similarities between different codes, and it would be perfectly possible to classify the morals of the most primitive peoples by the aid of categories which have universal significance.' (M. Ginsberg, *Reason and Unreason in Society*, pp. 302–303.)

(ix) *Variety of moral philosophies and the possibility of a true system*

The argument against the inference of ethical relativism from the historical variety of ethical systems follows the pattern of the previous criticism. Historical examination of any department of knowledge would show that opinions about what is true steadily changed, without impairing the contention that truth is not a myth. (Conversely, acceptance of an opinion is not a proof that it is true : Aristotle's physics was accepted as true for ages, yet it had to give place to new theories.) What is true is not necessarily manifest to everybody, for truth is often constructed out of a complex of intricate elements. Possibly, the case of ethics may be even more intricate than that of physics, and this could be a partial explanation for the prevalent disagreements in ethical theory. But the very disagreement does not prove the case of the relativist.[1] Nor does the fact that the conflicting opinions are those of philosophers (say, Kant and Mill), and not merely of savage peoples. If a mind like Aristotle's could be mistaken, it seems that anybody can be wrong.

It should be added that the controversy between *some* moral philosophers is not perhaps as profound as it superficially seems. Possibly, to give an example, formalism in ethics may allow of synthesis with some forms of hedonism, each doctrine stressing the problem of moral judgment from a different angle. This is, of course, a mere hint, which cannot be elaborated here.

(x) *Majority-rule and generally valid values*

To infer from the procedure of majority-rule that value-judgments are relative seems to us mistaken as well. Democratic theory may assume one objectively valid good, and nevertheless advocate determination of political action by majority vote, unstable though it may be. The explanation of this seeming contradiction would be that the right decision in each specific case is quite difficult to attain, because of the intricacy of social relations and of the dynamics of social change. To be sure, public opinion and political parties need not necessarily agree even on ultimate values, but perhaps more often their disagreement is due to differences of opinion in respect of the *means* to be adopted for the attainment of ends, and of the estimation of the advantages and disadvantages of the different means. The difficulties of determining the values and the means for their attainment may be quite serious, but they do not prove the contention that there are no true values.

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[1] Cf. the following passage from Weber : ' At first, I might make a few remarks against the view that the mere existence of historical and individual variations in evaluations proves the necessarily " subjective " character of ethics. Even propositions about empirical facts are often very much disputed and there might well be a much greater degree of agreement as to whether someone is to be considered a scoundrel than there would be (even among specialists) concerning, for instance, the interpretation of a mutilated inscription. ' (' The Meaning of " Ethical Neutrality " ', op. cit., p. 12–13.)

Democratic procedure, therefore, far from necessitating the acceptance of ethical relativity, takes notice of the difficulties in determining the desirable social action. This implies (or may imply) majority-rule not as a supreme goal, but as a means of social-political experimentation. Indeed it has been stressed[1] that democratic procedure is the application of the empiricism and tentativeness of scientific methodology to the sphere of social and political issues. Such an approach does not abolish scientific truth, and it should not impair our belief in political good.

It seems, therefore, that democracy need not imply ethical relativism, for it regards the democratic *changing* majority-rule merely as an imitation of scientific tentativeness, the end remaining approximation to the objectively desirable good.

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[1] See the stimulating essay of Bertrand Russell on ' Philosophy and Politics ', *Unpopular Essays*, London, 1950.

DEGREES OF EXPLANATION *

F. A. HAYEK

I

THE discussion of scientific method has been guided almost entirely by the example of classical physics. The reason for this is mainly that certain features of scientific method can be most easily illustrated by instances from this field, and partly a belief that, because physics is the most highly developed of all the empirical sciences, it ought to be held up to all the others for imitation. Whatever truth there may be in this second consideration ought not, however, to make us overlook the possibility that some of the characteristic procedures of physics may not be of universal applicability, and that the procedure of some of the other sciences, ' natural ' or ' social ', may differ from that of physics, not because the former are less advanced, but because the situation in their fields differs in significant respects from that of physics. More particularly, what we regard as the field of physics may well be the totality of phenomena where the number of significantly connected variables of different kinds [1] is sufficiently small to enable us to study them as if they formed a closed system for which we can observe and control all the determining factors ; and we may have been led to treat certain phenomena as lying outside physics precisely because this is not the case. If this were true it would certainly be paradoxical to try and force methods made possible by these special conditions on disciplines regarded as distinct because in their field these conditions do not prevail.

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* Received 8. xi. 54. I am indebted to Chester Barnard, Heinrich Klüver, Herbert Lamm, Michael Polanyi, Karl R. Popper, Warren Weaver and the members of a Faculty Seminar of the Committee on Social Thought in the University of Chicago for reading and commenting upon an earlier draft of this paper, though I have not been able to accept all their objections and criticisms.

[1] Modern physics has of course resorted to statistics to deal with systems with very large numbers of variables, but this does not seem to me to be in conflict with the observations in the text. The statistical technique is in effect a way of reducing the number of separate entities to which recognisable laws apply to comparatively few (the statistical collectives) and not a technique for dealing with the interplay of large numbers or such individual variables as individuals. The problems of complexity to which the further discussion refers are those which Weaver has described as ' problems of organised complexity ' in contradistinction to those ' problems of disorganised complexity ' with which we can deal with the methods of statistics Warren Weaver, ' Science and Complexity ', *American Scientist*, 1948, **36**, 4

For our attempt to bring out certain aspects of scientific method that are not generally appreciated we will start from the now widely accepted interpretation of theoretical science as a 'hypothetico-deductive' system. One may accept most of the basic ideas underlying this approach and yet feel that it can be interpreted in a manner which makes it inappropriate to some subjects. Its basic conception lends itself to a somewhat narrow interpretation according to which the essence of *all* scientific procedure consists in the discovery of *new* statements ('natural laws' or 'hypotheses') from which testable predictions can be derived. This interpretation may become a serious bar to the penetration of our understanding into fields where certainly at present, and perhaps forever, a different procedure may be our only effective means for obtaining guidance in the complex world in which we live.

The conception of science as a hypothetico-deductive system has been expounded by Popper in a manner which brings out clearly some very important points.[1] He has made it clear that the theoretical sciences are all essentially deductive, that there can be no such logical procedure as 'induction' which leads with necessity from the observation of facts to the formulation of general rules, and that the latter are products of creative acts of the mind which cannot be formalised. He has also emphasised the important point that the conclusions to which theories lead are essentially of the nature of prohibitions: they 'forbid' the occurrence of certain kinds of events and can never be definitely 'verified' but only increasingly confirmed by persistently unsuccessful attempts to prove them false. For what follows this part of the argument will be accepted.

There is, however, a further and no less illuminating idea contained in this approach which, if accepted too literally, is apt to become misleading. It is what Popper has occasionally expressed in conversation[2] by saying that science does not explain the unknown by the known, as is commonly believed, but, on the contrary, the known by the unknown. What is meant by this apparent paradox is that the advance of knowledge consists in the formulation of new statements which often refer to events which cannot be directly observed and from which, in combination with other statements about particulars, we can derive statements capable of disproof by observation. I do not doubt that it is important to emphasise that additions to knowledge in such instances will be contained in new statements (hypotheses or natural laws) which form part of the basis of our deductive argument ; but this seems to me to represent not a general characteristic of all scientific procedure, but one which may be the rule in physics and occasionally also be successful in the biological sciences, but which presupposes conditions which are not present in many other fields.

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[1] Although on some particular points Professor Popper has in recent publications ('The Poverty of Historicism', *Economica*, 1945, **11** & **12**; *The Open Society*, 1950) improved on his formulations, it is still necessary to go for a full account to his *Logik der Forschung*, 1935, unfortunately not yet available in English. In many respects what follows is little more than an elaboration of some of Popper's ideas, particularly of his conception of degrees of testability and of his 'relativisation' of his falsifiability criterion. My critical observations are therefore directed solely against certain positivist and operationalist interpretations of the 'hypothetico-deductive' thesis but not against Popper's or similar variants.

[2] The nearest approach to this statement which I can find in print occurs in Popper, 'The Nature of Philosophical Problems and their Roots in Science', this *Journal*, 1952, 3, 124

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Even in so far as the physical sciences are concerned, the emphasis on the procedure from the hypothesis to be tested to the conclusions which can be proved false may go too far. A large part of the value of these disciplines undoubtedly derives from the fact that once their hypotheses are well accredited we can confidently derive from them conclusions applicable to new circumstances and treat these as true without testing them. The work of the theorist is not concluded when his hypotheses seem sufficiently confirmed. The activity of thinking through all their implications is evidently an activity important and valuable in its own right ; and it may sometimes be an activity of great complexity and difficulty, requiring the highest forms of intelligence. Nobody will deny that constant efforts in this direction are part of the regular task of science ; in fact, whole theoretical disciplines are concerned almost exclusively with this kind of activity. The question of what is the range of application or the capacity of a theory, whether it can or cannot account for a certain group of observed phenomena, or whether the observed events are within the range of what might have been predicted from it if all the relevant factual data had been known and if we were capable of manipulating them adequately, is often as interesting a problem as that whether the particular conclusion derived from the theory can be confirmed ; and it is clearly independent of that question.

These aspects of the work of the theorist become increasingly prominent as we turn from the ‘ pure ’ theory of physics to disciplines like astrophysics or the various branches of geophysics (seismology, meteorology, geology, oceanography) which are sometimes described as ' applied ' sciences. This name hardly describes the distinct kind of effort which those disciplines involve. It is used here neither to express that, like technology, they serve particular human needs, nor in order to indicate that their applicability is confined to particular regions of time and space. They all aim at developing generic explanations which, at least in principle, are significant apart from the particular events for which they have been worked out : much of the theory of tides as developed in terrestrial oceanography would be applicable to oceans on Mars, etc. What is characteristic of these theories is that they are, in a sense, derivative : they consist of deductions derived from combinations of known laws of physics, and do not, strictly speaking, state distinct laws of their own but elaborate the laws of physics into explanatory patterns appropriate to the peculiar kind of phenomena to which they refer. It is, of course, conceivable that the study of the tides might lead to the discovery of a new natural law ; but if it did it would presumably be a new law of physics and not one of oceanography. Yet oceanography will still contain general statements which are not just plain physics but which have been elaborated from the laws of physics in order to account for the joint effects of certain typical constellations of physical events—specific patterns of argument developed to deal with recurring types of situations.

It is, no doubt, desirable that in working out such deductive systems the conclusions should be tested against the facts at every step. We can never exclude the possibility that even the best accredited law may cease to hold under conditions for which it has not yet been tested. But while this possibility always exists, its likelihood in the case of a well confirmed hypothesis is so small that we often disregard it in practice. The conclusions which we can draw from a combination of well established hypotheses will therefore be valuable though we may not be in a position to test them.

In a certain sense such a deductive argument developed to account for an observed phenomenon does not contain *new* knowledge. To those who are not regularly concerned with the elaboration of such patterns of explanation for typical complex situations the tasks of merely deducing the combined effects of known laws may seem trivial. But this is true only in the sense in which it is also true of mathematics. That certain conclusions are implied by what we know already does not necessarily mean that we are aware of these conclusions or are able to apply them whenever they would help us to explain what we observe. Nobody could, in fact, work out all the consequences implied by our existing knowledge or even of some of the most trivial and undoubted propositions which we employ in daily life ; it will often be an exceedingly difficult task to decide how much of what we observe can be explained by laws already known, or could be so explained if we possessed all the relevant data. To squeeze out of what we already know as many significant conclusions as possible is, of course, not a purely deductive task : it must be guided by observation in its choice of problems. But, though observation will raise the problems, the answer will rest on deduction alone.

In the disciplines mentioned the important question, thus, usually is not whether the hypotheses or laws used for the explanation of the phenomena are true, but whether we have selected the appropriate hypotheses from our store of accepted statements and have combined them in the right manner. What will be new about such a ' new ' explanation of some phenomena will be the particular combination of theoretical statements with statements about facts regarded as significant for the particular situation (the ' initial ' and ' marginal conditions '), not any one of the theoretical statements from which it starts. And the problem will not be whether the model as such is true, but whether it is applicable to (true *of*) the phenomena it is meant to explain.

We have up to this point spoken mainly of what are called applied branches of physics in order to show that even here much of the undoubtedly theoretical work does not aim at the discovery of new laws and at their confirmation, but at the elaboration from accepted premisses of deductive patterns of argument which will account for complex observed facts. If in these instances we can speak of hypotheses which require to be tested, they must be sought in the assertion that this or that pattern fits an observable situation, and not in the conditional statements of which the explanatory pattern itself consists and which is assumed to be true. We shall later discuss the peculiarities of this procedure more fully. At present our aim was merely to stress how comparatively rare an event in the progress even of the physical sciences the discovery of a true new law of nature is, and to suggest how special may be the conditions under which we can hope to discover such new laws of nature.

3

By a scientific prediction we mean the use of a rule or law in order to derive from certain statements about existing conditions statements about what will happen (including statements about what we will find if we search at a particular point). Its simplest form is that of a conditional or ' if then ' statement combined with the assertion that the conditions stated in the antecedent are satisfied at a particular time and place. What in this connection is usually not explicitly considered is how specific need be the description of the events mentioned in the law, in the statement of the initial and marginal conditions, and in the prognosis, in order to merit the name of prediction. From the simple examples commonly adduced from physics it is readily concluded that it will generally be possible to specify all those aspects of the phenomenon in which we are interested with any degree of precision which we may need for our purposes. If we represent this form of statement by ' if u and v and w then z ', it is often tacitly assumed that at least the description of z will contain all the characteristics of z which are deemed significant for the problem in hand. Where the relations we are studying are between a comparatively small number of measurements, this appears to present no serious difficulties.

The situation, however, is different where the number of significantly interdependent variables is very large and only some of them can in practice be individually observed. The position will here frequently be that *if we already knew* the relevant laws, we could predict that if several hundred specified factors had the values $x_1, x_2, x_3, \ldots x_n$, then there would always occur $y_1, y_2, y_3, \ldots y_m$. But in fact all that our observation suggests may be that if x_1, x_2, x_3, and x_4, then there will occur either (y_1 and y_2) or (y_1 and y_3) or (y_2 and y_3), or some similar situation—perhaps that if x_1, x_2, x_3, and x_4, then there will occur some y_1 and y_2 between which either the relation P or the relation Q will exist. There may be no possibility of getting beyond this by means of observation, because it may in practice be impossible to test all the possible combinations of the factors $x_1, x_2, x_3, x_4, \ldots x_n$. If in the face of the variety and complexity of such a situation our imagination cannot suggest more precise rules than those indicated, no systematic testing will help us over the difficulty.

In situations like these the observation of the complex facts will therefore not enable us to invent new hypotheses from which we can deduce predictions for situations we have not yet observed. We shall not be in a position to discover new natural laws for the kind of complex in question which would enable us to arrive at new predictions. The current view often seems to regard such a situation as beyond the limits of the application of scientific method (at least for the existing state of observational technique) and to accept that for the time being science must stop there. If this were correct, it would be very serious. There is no guarantee that we shall ever be able, physically or conceptually, to handle phenomena of any degree of complexity, nor that phenomena of a degree of complexity exceeding the limit may not be very important.

But if there is no reason to assume that the conditions presupposed by the standard method of physics will be satisfied by all events in which we are interested, there is still no need to despair about our prospects of learning at least something of importance about phenomena where they are not satisfied. But this will require a kind of reversal of what has been described as the standard procedure of physics ; we shall here have to proceed in our deductions, not from the hypothetical or unknown to the known and observable, but—as used to be thought to be the normal procedure—from the familiar to the unknown. This is not an entirely satisfactory description of the procedure we shall now have to examine ; but it is still true that the older conception of explaining the new by the familiar fits this procedure better than the conception that we proceed from the unknown to the known.

4

'Explanation'[1] and 'prediction' of course never refer to an individual event but always to phenomena of a certain *kind* or class ; they will state only some and never all the properties of any particular phenomenon to which they refer. In addition, each property stated will be expressed not as a unique value or magnitude but as a range, however narrow, within which the property will fall. Because of the limitations of the possible precision of measurement this is true even of the most exact predictions of physics which, strictly speaking, never say more than that the magnitude in question will fall within a certain interval; and it is still more obviously the case where the prediction is not quantitative.

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[1] I assume that the prejudice of certain earlier positivists against the word 'explanation' is now a thing of the past and that it may be taken for granted that prediction and explanation are merely two aspects of the same process where, in the first instance, known rules are used to derive from the known facts what will follow upon them, while in the second instance these rules are used to derive from the known facts what preceded them. For the purposes of this article it would indeed make no important difference if instead of 'degrees of explanation' we spoke throughout of 'degrees of prediction'. Cf. K. R. Popper, *The Open Society*, 1950, p. 446.

In ordinary usage we are inclined to admit as predictions only statements which narrow down the admitted phenomena fairly closely, and to draw a distinction between ' positive ' predictions such as ' the moon will be full at 5h 22′ 16″ tomorrow, and merely negative predictions such as ' the moon will not be full tomorrow '. But this is no more than a distinction of degree. Any statement about what we will find or not find within a stated temporal and spatial interval is a prediction and may be exceedingly useful : the information that I will find no water on a certain journey may indeed be more important than most positive statements about what I will find. Even statements which specify no single specific property of what we will find but which merely tell us disjunctively that we will find either x or y or z must be admitted as predictions, and may be important predictions. A statement which excludes only one of all conceivable events from the range of those which may occur is no less a prediction and as such may prove to be false.

5

Where we have to deal with a complex situation in which observation discloses only very limited regularities, be it in the ' applied ' branches of physics or in biology or in the social sciences, we usually ask to what extent our existing knowledge of the forces at work, or of the properties of some of the elements of the complex, may account for what we observe. We endeavour to find out whether this may be derived by deduction from what we know about the behaviour under simpler conditions of some of the factors involved. Of course we can never be certain that what we know about the action of those forces under simpler conditions will apply to more complex situations, and we will even have no direct way of testing this assumption, since our difficulty is precisely that we are unable to ascertain by observation the presence and specific arrangement of the multiplicity of factors which form the starting point of our deductive reasoning. Neither the assumption that factors of the kind assumed are present, nor of course the validity of the deductive reasoning, need, therefore, be regarded as disproved if the conclusions at which we arrived are not born out by observation. But though observation of such complex situations cannot decide whether our conditional (' if-then ') argument is true, it will help us to decide whether to accept it as an explanation of the facts which we observe.

It will be of interest, of course, if we succeed in deducing from our premisses precisely these partial regularities of the complex from which we started. But this, though it may give us satisfaction, does not add to our knowledge. Yet the assertion that what we observe is due to a certain constellation of familiar factors, though we may not be able to test it directly, will usually imply consequences which we can test. The mechanism which we believe to have produced the observed phenomena will be capable of producing some further results but not others. This means that if what we have observed of a given complex of events is due to the assumed mechanism, that complex will also possess certain other characteristics and not be capable of definite other kinds of behaviour. Our tentative explanation will thus tell us what *kinds* of events to expect and which not, and it can be proved false if the phenomena observed show characteristics which the postulated mechanism could not produce. It will thus give us new information by indicating the *range* of phenomena to expect. By providing a schema or framework for the possible results it not only helps us to order the observational knowledge which we already possess, but will also provide *niches* for new observations likely to occur, and indicate the directions in which we must expect the phenomena to vary. Not only will the observed facts thus come to ' make sense ' and to ' fall into their places ', but we shall be able to make predictions about the combinations of events which will not occur if our explanation is correct.

This procedure differs from the supposedly normal procedure of physics in that we do here not *invent* new hypotheses or constructs but merely *select* them from what we know already about some of the elements of the phenomena ; in consequence we do not ask whether the hypotheses we used are true or whether the constructs are appropriate, but whether the factors we have singled out are in fact present in the particular phenomena we want to explain, and whether they are relevant and sufficient to explain what we observe. The answer will depend on whether what we observe is of the kind which according to our deductions would occur *if* the postulated factors were present.

6

The most familiar instance in the natural sciences of this sort of mere 'explanation of the principle'[1] is probably provided by the theory of evolution by natural selection of the different organisms. It is a theory which neither aims at specific predictions of particular events, nor is based on hypotheses in the sense that the several statements from which it starts are expected to be confirmed or refuted by observation. Although, as is true of any scientific theory, it does delimit a range of facts which are permitted by it against others which it 'forbids', our purpose in examining the facts is not to ascertain whether the different individual premisses from which the theory starts are true, but to test whether the particular combination of undoubted premisses is adequate to arrange the known facts in a meaningful order and (what in a sense is the same thing) to show why only certain kinds of events are to be expected while others are precluded.

However we prefer to phrase the individual premisses from which we deduce the theory of evolution, they will all be of a kind that we do not doubt their truth and should not regard them as refuted if the conclusions drawn from them jointly should be contradicted by observation. We can get a considerable distance by starting from the following three assumptions: (i) Organisms which survive to the reproductive stage produce on the average a number of offspring much greater than their own; (ii) While organisms of any one kind produce as a rule only similar organisms, the new individuals are not all completely similar to their parents, and any new properties will in turn be inherited by their offspring; and (iii) some of these mutations will alter the probability that the individuals affected will in turn produce offspring.[2]

Few people will doubt that these statements are true, or believe that the problem of the theory of evolution is whether they are true or not. The problem is rather whether they are adequate and sufficient to account for the phenomena which we do observe and for the absence of others which do not occur. We want to know what this mechanism of reduplication with transmittable variations and competitive selection can achieve, and this question we can answer only by deductively working out all implications of these assumptions. We shall accept the conclusions drawn from the premisses and regard them as a satisfactory explanation if they not only allow us to derive from them a process by which the observed phenomena might have been brought about, but if it also points to new (not yet observed) distinctions between what is and what is not possible which are later confirmed by observation.[1]

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[1] Though this term is rarely defined, theoretical discussion in biology abounds with statements qualified by the addition of 'in principle', such as 'is in principle specifiable,' 'can in principle be ascertained', 'such a reduction is in principle possible,' etc. Cf. A. S. Sommerhoff, *Analytical Biology*, London, 1950, pp. iv, v, 27, 30, 180.

[2] For a similar listing of these basic assumptions see J. S. Huxley, *Evolution*, London, 1942, p. 14.

In some instances such a theory may in fact produce practically no new conclusions but will merely provide a rational foundation for the biologist's knowledge that ' nature does not work that way '. It has even been suggested of the theory of evolution by natural selection that the main objection to it is that it cannot be disproved because ' it appears impossible to indicate any biological phenomena that would plainly disprove it '.[2] This is true only in a limited sense. The individual statements from which it is derived are indeed unlikely to be disproved. But the assertion that the observed differentiation of species is always due to the operation of these factors could be refuted, e.g. if it were observed that after a sudden change in the environment the individuals then living would at once begin to produce offspring possessing a new adaptation to the changed environment. And, in the form in which the premisses have been stated before, their adequacy as an explanation has in fact been shown insufficient by the inheritance of specific attributes of the non-sexual members of certain types of social insects. To account for these, the premisses have to be enlarged to include situations wherein not only the properties of the individual but also properties of other members of the group will affect the chances of successful procreation.

It is worthwhile to pursue a little further the question how much the theory of evolution explains or predicts, and what the causes are of the limitations to what it can do. It can explain or predict only

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[1] A very neat statement of the relation between theory and observation in this field, which is of wider application, occurs in G. S. Carter, *Animal Evolution*, London, 1951, p. 9 : ' The palaeontologist may be able to exclude some theories of evolution on the ground that they demand change not in accord with the facts ; he claimed to be able to do so for Mendelian theories in their form at the start of this century. . . The part of palaeontology in the study of evolution resembles that of natural selection in the process of evolution ; it serves to remove the inefficient, but, cannot itself initiate.' See also Popper, ' The Poverty of Historicism ', as cited.

[2] L. von Bertalanffy, *Problems of Life*, New York, 1952, p. 89

kinds of phenomena, defined by very general characteristics: the occurrence, not at a narrowly defined time and place but within a wide range, of changes of certain *types*; or rather the absence of other types of changes in the structure of the succeeding organisms. Disputes which have arisen in the course of the growth of the theory of evolution have thus significantly turned not so much about facts but about such questions as whether the postulated mechanism can account for the evolution having taken place in the time which has been available. And the answer has frequently come, not from the discovery of new facts, but from purely deductive argument such as the mathematical theory of genetics, while ' experiment and observation did not quite keep up with the mathematical theory of selection '.[1] If we can test the deductions by observation, so much the better: if we conclude, e.g. that mice of a colour little different from that of the ground are less likely to be caught by owls and will therefore multiply more rapidly than those with a contrasting colour and ultimately dominate the species, it is no doubt desirable that we should be able to confirm this by experiment (as has been done); because it is at least conceivable that such a tendency may be counteracted by another, e.g. by the frequent losses to owls stimulating the fecundity of the species affected (as the proportion of male births among humans was once believed to increase in wartime). But even if such direct confirmation by experiment is not possible, it will be reasonable to accept the deductive conclusions until they are disproved.

7

The kind of explanation with which we are concerned is in current discussion often referred to as ' model-building '. This expression does not emphasise quite the distinction with which we are concerned since even the most precise predictions of physics are based on the use of ' models ' of a formal or material kind.[2] But if the term model is meant to stress that a model always represents only some but not all the features of the original (so that an exact replica of a machine could not appropriately be called a model), it indeed brings out an important feature which all explanations possess but to very different degrees.

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[1] L. von Bertalanffy, *Problems of Life*, New York, 1952, p. 83

[2] Cf. A. Rosenblueth and N. Wiener, ' The Rôle of Models in Science ', *Philosophy of Science*, 1945, **12**, 317: ' A material model is the representation of a complex system by a system which is assumed simpler and which is also assumed to have some properties similar to those selected for study in the original complex system. A formal model is a symbolic assertion in logical terms of an idealized relatively simple situation showing the structural properties of the original factural system.' In connection with what follows see also K. W. Deutsch, ' Mechanism, Organism, and Society ', *Philosophy of Science*, 1951, **18**, 3; ' Mechanism, Teleology, amd Mind ', *Philosophy and Phenomenological Research*, 1952, **12**, 185

This difference of degree is well illustrated by the suspicion with which the physicist frequently regards the formal models employed in the biological and social sciences. To the physicist the value of a model (especially of a mathematical model represented by a set of equations) normally consists in the fact that he can ascertain and insert the relevant variables and thus derive the quantitative values of the events to be predicted or explained. Yet in the disciplines mentioned similar models are regularly used although the values of the variables cannot in fact be ascertained, and often though there is no prospect of ever ascertaining them. Yet explanatory value is claimed for these models irrespective of this possibility, i.e. although they do not enable us to predict that such and such a specific event will occur at a particular time and place. Wherein does this explanatory value consist ?

The answer should now be obvious. Any model defines a certain range of phenomena which can be produced by the type of situation which it represents. We may not be able directly to confirm that the causal mechanism determining the phenomenon in question is the same as that of the model. But we know that, *if* the mechanism is the same, the observed structures must be capable of showing some kinds of action and unable to show others ; and if, and so long as, the observed phenomena keep within the range of possibilities indicated as possible, that is so long as our expectations derived from the model are not contradicted, there is good reason to regard the model as exhibiting the principle at work in the more complex phenomenon.

The peculiar thing about these kinds of models is that, because we have to draw deductions from what we know about some factors contributing to the phenomenon and know nothing about others, our conclusions and predictions will also refer only to some properties of the resulting phenomenon, or, in other words, to a *kind* of phenomenon rather than to a particular event. Strictly speaking, as we have seen, this is true of all explanations, predictions, or models. Yet there is of course a great difference between the prediction that upon turning a switch the pointer of a measuring instrument will be at a particular figure and the prediction that horses will not give birth to hypogryphs or that, if all commodity prices are fixed by law and demand afterwards shifts, people will not be able to buy as much of every commodity as they would wish to buy at these prices.

If we consider a formal model consisting of a system of algebraic equations or 'propositional equations',[1] it will contain assertions about a structure of relations even if we do not know the value of any of the variables and even if we have only the most general information about the character of the functions occurring in it: it will still exclude the possibility of the occurrence of certain combinations of values in any phenomenon which the model is asserted to represent;[2] it will tell us both what combinations of variables can occur at any time and what range of values the other variables can assume when the value of one or more of the variables is known. Of course, as we become able to insert more and more definite values for the variables, this range will be narrowed until we reach the point when the system is completely determined and only one value of the remaining variable possible.

It is often not recognised that even the most formal system of equations can thus be used to make predictions and therefore will possess empirical content (though this content would be small), and that it will thus provide an explanation of the common features of a wide range phenomena—or an explanation of the principle of this kind of phenomenon. This needs to be stressed because of the widespread misconception that the value of such models rests entirely on our ability to specify the values of the variables occurring in them and that they are useless so long as we cannot do this. This is not so: such models are valuable on their own, irrespective of their use for determining particular situations, and even where we know that we shall never have the information which would make this possible. They still do tell us something about the facts and allow us to make prognoses.

But is it not still true that our aim everywhere, as has been said of the theoretical description of nature,[3] should be to formulate theories that can be 'falsified' as easily as possible, i.e. which have as great an empirical content as possible? It is undoubtedly a drawback to have to work with theories which can be refuted only by statements of a high degree of complexity, because anything below that degree of complexity is on that ground alone permitted by our theory.[1] Yet it is still possible that in some fields the more generic theories are the more useful ones and further specification may be of little practical value. Where only the most general patterns can be observed in a considerable number of instances, the endeavour to become more 'scientific' by further narrowing down our formulae may well be a waste of effort; to strive for this in some subjects such as economics has often led to the illegitimate assumption of constants where in fact we have no right to assume the factors in question to be constant.

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[1] i.e. propositional functions for whose variables we will admit only values which make the propositions true. See K. R. Popper, *Logik der Forschung*, 1934, p. 36.

[2] K. R. Popper, ibid. 'Even if the system of equations is not sufficient to determine a unique solution, we cannot substitute any possible combination of values for its variables, it singles out a certain class of systems of values as admissible and another class as not admissible.' (Translated from the original.)

[3] Popper, op. cit., p. 68

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Though our conclusions are most readily seen to apply to those disciplines which, like mathematical biology or mathematical economics, employ formalised symbolic models, they are no less true of those biological and social theories which are expressed in ordinary language. While it would be equally incorrect, however, to say that these theories do not lead to predictions, and while their value does indeed rest on what they predict, it must be recognised that those predictions are so different in character from what is usually understood by this word that not only the physicist but also the ordinary man may well hesitate to accept them as such. They will be mostly negative predictions that such and such things will not occur, and more especially predictions that such and such phenomena will not occur together. These theories equip us with ready-made schemes which tell us that when we observe given patterns of phenomena, certain other patterns are to be expected but not some others. They will show their value through the manner in which the isolated facts which have been known will begin to make sense, will fill the *niches* which the theory provides, and only those. In some respects such theories may seem little more than schemes of classification, yet schemes which provide in advance only for such phenomena or combinations of phenomena as the theories allow to occur. They indicate the range of phenomena to be expected: if the taxonomic scheme of zoology does not provide for winged vertebrates with more than two legs this is the result of a theory which makes it unlikely that such organisms have arisen. If economics tells us that we cannot at the same time maintain fixed rates of foreign exchange and at will control the internal price level of a country by changing the quantity of money, the character of these ' predictions ' is essentially the same. It is, in particular, because its predictions possess this character that economics appears so often to consist merely of variations upon the theme that ' you cannot have your cake and eat it '. The practical value of such knowledge consists indeed largely in that it protects us from striving for incompatible aims. The situation in the other theoretical social sciences, such as theoretical anthropology, seems to be very much the same : what they tell us is in effect that certain types of institutions will not be found together, that because such and such institutions presuppose certain attitudes on the part of the people (the presence of which can often not be confirmed satisfactorily), only such and such other institutions will be found among people possessing the former (which can be confirmed or refuted by observation).

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[1] Popper, op. cit, p. 80

The limited character of the predictions which these theories enable us to make should not be confused with the question whether they are more or less uncertain than the theories which lead to more specific predictions. They are more uncertain only in the sense that they *leave* more uncertain because they say less about the phenomena, not in the sense that *what* they say is less certain. In so far as the latter sometimes may also be the case, it will be due to a different factor with which we are not here concerned : where we deal with very complex phenomena the *recognition* of the presence of the conditions to which the theory applies may often require the ready perception of patterns or configurations which will demand a special skill which few acquire. The selection and application of the appropriate theoretical scheme thus becomes something of an art where success or failure cannot be ascertained by any mechanical test.[1] The possession of such a ready-made pattern of significant relationships gives us a sort of sense for the physiognomy of events which will guide us in our observation of the environment. But even this constitutes no more than a distinction of degree from the physical sciences : the reading of many instruments also requires very special skills and there will be no other test for its correctness than that the great majority of properly trained observers agree.

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[1] This is perhaps the place to mention that what we are discussing here is of course not the only difference between the physical and the social sciences, but rather a peculiarity which the latter share with those natural sciences which deal with comparatively complex phenomena. Another and perhaps more important peculiarity of the social sciences is due to the fact that here the *recognition* of the different kinds of facts rests largely on a similarity between the observer and the observed persons.

9

The service of a theory which does not tell us what particular events to expect at a definite moment, but only what kinds of events we are to expect within a certain range, or on complexes of a certain type, would perhaps be better described by the term *orientation* than by speaking of prediction. Although such a theory does not tell us precisely what to expect, it will still make the world around us a more familiar world in which we can move with greater confidence not to be disappointed because we can at least exclude certain eventualities. It makes it a more orderly world in which the events make sense because we can at least say in general terms how they hang together and are able to form a coherent picture of them. Though we are not in a position to specify precisely what to expect or even to list all the possibilities, each observed pattern has meaning in the sense that it limits the possibilities of what else may occur.

Where our predictions are thus limited to some general and perhaps only negative attributes of what is likely to happen, we evidently also shall have little power to control developments.[1] Yet the knowledge of what kinds of events are to be expected and what not, will still help us to make our action more effective. Even if we cannot control the external circumstances at all, we may adapt our actions to them. And sometimes, though we may not be able to bring about the particular results we would like, knowledge of the principle of the thing will enable us to make circumstances more favourable to the kinds of events we desire. Of the different classes of events which are to be expected under various combinations of circumstances which we can bring about, some may with greater probability include desirable results than others. An explanation of the principle will thus often enable us to create such favourable circumstances even if it does not allow us to control the outcome. Such activities in which we are guided by a knowledge merely of the principle of the thing should perhaps better be described by the term '*cultivation*', than by the familiar 'control'—cultivation in the sense in which the farmer or gardener cultivates his plants, where he knows and can control only some of the determining circumstances, and in which the wise legislator or statesman will probably attempt to cultivate rather than control the forces of the social process.

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[1] While it is evidently possible to predict precisely without being able to control, we shall clearly not be able to control developments further than we can predict the results of our action. A limitation of prediction thus implies a limitation of control, but not *vice versa*.

MAXWELL ON THE METHOD OF PHYSICAL ANALOGY *

JOSEPH TURNER

IN addition to his scientific work, James Clerk Maxwell gave some attention to what is now known as the philosophy of science. Much of what he said has been said by others, but he did say it early and rather well. Maxwell was interested in both broad philosophical issues and the special problems which arose out of his own contribution to physics. These special problems included physical analogy, dynamical explanation, and action at a distance. The present report is confined to Maxwell's views on the method of physical analogy.

There are fashions in scientific explanation. Nineteenth-century Cambridge physics had its own distinctive cut, but Maxwell's observations on the methods he employed and the results he obtained are still instructive today. Maxwell did not write systematically on physical analogy. His views have to be gathered from the many asides that are scattered through his scientific memoirs and treatises, and from the more popular papers he prepared. These papers include lectures, addresses to learned societies, biographical sketches, articles for the Ninth Edition of the *Encyclopædia Britannica,* and book reviews for *Nature.*[1]

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* 26. vii. 54. In working out the substance of this paper I wish to acknowledge the help received from Professors John C. Cooley and Ernest Nagel, and the suggestions offered by Sidney Morgenbesser, A. C. Danto, Leo Litwak, and Shirley Danto.

[1] The scientific memoirs together with the papers of general interest are collected in the *The Scientific Papers of James Clerk Maxwell*, ed. by W. D. Niven, Cambridge, 1890. The treatises comprise *A Treatise on Electricity and Magnetism*, 3rd edn., Oxford, 1892 ; the *Elementary Treatise in Electricity*, ed. by William Garnett, Oxford, 1881 ; *Theory of Heat*, 8th edn., London, 1885 ; and, written for a wider audience, *Matter and Motion*, New York. Maxwell edited *The Electrical Researches of the Honourable Henry Cavendish, F.R.S.*, Cambridge, 1879. Some of Maxwell's letters and philosophical essays not written for publication are printed in Lewis Campbell and William Garnett, *The Life of James Clerk Maxwell*, London, 1884, ; and in *Origins of Clerk Maxwell's Electric Ideas,* ed. by Sir Joseph Larmor, Cambridge, 1936.

References to the *Papers* will be by volume and page ; references to *Electricity and Magnetism* and the *Elementary Treatise* will be by article ; references to the remaining works will be by page. The *Papers* and *Electricity and Magnetism* as well as *Matter and Motion* have recently been reprinted by Dover.

I

In an address delivered in 1870 to the British Association, Maxwell characterised physical analogy as a similarity in mathematical form between two branches of science which differed in the physical nature of the phenomena they described. He also referred to physical analogy as scientific illustration and as scientific metaphor.

> Now a truly scientific illustration is a method to enable the mind to grasp some conception or law in one branch of science, by placing before it a conception or a law in a different branch of science, and directing the mind to lay hold of that mathematical form which is common to the corresponding ideas in the two sciences, leaving out of account for the present the difference between the physical nature of the real phenomena.[1]

In the course of his writings Maxwell made three points that served to clarify the nature or character of physical analogy. One, he found physical analogy in scientific explanation to be the mean between pure mathematical formulae, on the one hand, and physical hypotheses, on the other. Two, he distinguished two senses in which analogies are incomplete, that is, two senses in which the similarity between the branches of science compared is only partial. Three, he discussed the possibility of a new language suitable to the exact, yet economical, expression of different physical analogies. Maxwell's comments were not formal. He both made these points and drove them home by means of well chosen illustrations.

The account of physical analogy as a mean between the excesses of mathematical description and physical hypothesis appeared in Maxwell's very first memoir on electricity and magnetism, his paper ' On Faraday's Lines of Force ', published in 1855. The opening paragraph complained that the mathematical study of electricity had become so complicated that its mere retention in the memory hindered the further progress of that discipline. Simplification was necessary.

> The results of this simplification may take the form of a purely mathematical formula or of a physical hypothesis. In the first case we entirely lose sight of the phenomena to be explained ; and though we may trace out the consequence of given laws, we can never obtain more extended views of the connections of the subject. If, on the other hand, we adopt a physical hypothesis, we see the phenomena only through a medium, and are liable to the

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[1] ' Address to the Mathematical and Physical Sections of the British Association,' *British Association Report*, **40** ; reprinted in *Papers*, **2**, 219

> blindness of facts and rashness in assumption which a partial explanation encourages. We must therefore discover some method of investigation which allows the mind at every step to lay hold of a clear physical conception, without being committed to any theory founded on the physical science from which the conception is borrowed, so that it is neither drawn aside from the subject in pursuit of analytical subtleties, nor carried beyond the truth by a favourite hypothesis.[1]

The memoir used the analogy between light and wave motion in an elastic solid to illustrate this point. To strip the theory of light of its dressing, to reduce it to ' theory of " transverse vibration " ', is to offer a mathematical description. To ask what property of light it is that vibrates, or to seek a medium in which this vibration occurs, is to resort to physical hypothesis. But to exploit this comparison to the fullest and yet remember that it is but a similarity in mathematical form between two sciences, is to proceed by the method of physical analogy.[2]

The memoir ' On Faraday's Lines of Force ' applied this golden mean to the problems of electricity and magnetism. It translated some of Faraday's ideas into mathematical language, but it did not yet contain the general equations of the electromagnetic field. This discovery awaited Maxwell's second memoir, ' On Physical Lines of Force ', published in 1861-62. The translation of Faraday's ideas was accomplished, in part, by the demonstration of the existence of an analogy between what are now called stationary or steady fields and the steady motion of a frictionless, incompressible fluid. The fluid was imagined to move through a sponge-like substance which opposed its motion with a resistance proportional to the velocity of the fluid. Here in outline is this analogy for the electrostatic field.

The positively and negatively charged bodies are analogous respectively to the sources at which the ideal fluid is created and the sinks at which it empties. The lines of electric force are analogous to what Maxwell called ' tubes of fluid motion '. The lines of force indicate, by their direction and divergence (or relative compactness), the direction and intensity at any point of the electric force ; the tubes, running from the sources to the sinks, indicate, by their direction and cross sections, the direction and speed at any point of the fluid. The pressure exerted by the fluid in overcoming the resistance of the sponge-like substance is analogous to the electrostatic potential, while the substance itself is analogous to the dielectric in which the charged bodies are embedded.[1]

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[1] ' On Faraday's Lines of Force ', *Transactions of the Cambridge Philosophical Society*, **10**, Part I ; reprinted in *Papers*, **1**, 155-156

[2] Ibid., *Papers*, **1**, 156

The analogy is of historical importance because it clearly indicated that electrostatics could be investigated mathematically, not only in terms of Coulomb's inverse square law, but also in terms of Faraday's lines of force. The lines of electric force and tubes of fluid motion both satisfy the same partial differential equations. This analogy, by the way, was not the first attempt to compare Faraday's lines of force to more familiar notions. Kelvin, in 1842, when seventeen years old, in his paper ' On the Uniform Motion of Heat in Homogeneous Solid Bodies,' established an analogy between electrostatics and the flow of heat in an unevenly heated solid.[2]

The analogy between electrostatics and fluid motion was an application of Maxwell's golden mean. The fluid was not offered as a physical hypothesis nor the theory developed in purely mathematical terms. ' On Faraday's Lines of Force ' emphasised this fact.

> The substance here treated . . . is not even a hypothetical fluid which is introduced to explain actual phenomena. It is merely a collection of imaginary properties which may be employed for establishing certain theorems in pure mathematics in a way more intelligible to many minds and more applicable to physical problems than that in which algebraic symbols alone are used.[3]

And in another passage :

> In this outline of Faraday's electrical theories, as they appear from a mathematical point of view, I can do no more than simply state the mathematical methods by which I believe that electrical phenomena can be best comprehended and reduced to calculation,

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[1] ' On Faraday's Lines of Force ', *Transactions of the Cambridge Philosophical Society,* **10**, Part 1 ; reprinted in *Papers,* **1**, 160–175

[2] Lord Kelvin, ' On the Uniform Motion of Heat in Homogeneous Solid Bodies', *Cambridge Mathematical Journal*, 1842, **3**, reprinted in *Reprint of Papers on Electrostatics and Magnetism*, London, 1872, pp. 1-14. Maxwell did refer to Kelvin's work. Kelvin, in turn, in a note added to the memoir in 1854, stated that he was anticipated in some results by Chasles who was anticipated by Green. The analogy to heat, however, Kelvin claimed, was not previously established. In another note, added the same year, Kelvin drew attention to Faraday's use of phrases like ' conducting power ' in his discussion of lines of force. E. T. Whittaker in his *A History of the Theories of Aether and Electricity*, London, 1910, p. 269, remarks, however, that already in 1827 Ohm had found an analogy between the flow of heat and an electric current in a solid conductor, which is close to Kelvin's analogy.

[3] ' On Faradav's Lines of Force', *Papers*, **1**, 160

> and my aim has been to present the mathematical ideas to the mind in an embodied form, as systems of lines or surfaces, and not as mere symbols, which neither convey the same ideas, nor readily adapt themselves to the phenomena to be explained.[1]

Maxwell's researches in the kinetic theory of gases may be understood as the development of a physical analogy into a physical hypothesis. The system of particles came to 'constitute', as Maxwell said, the gas in question. If the analogy of the ideal fluid were likewise changed into a physical hypothesis, it would be something like the effluvia theories of electricity offered by Gilbert in the seventeenth century and Desagulier in the eighteenth century. These thinkers pictured electrostatic attraction and repulsion as due to an imponderable medium streaming out of the charged bodies to act by contact upon other charged bodies.[2]

The second point which served to clarify the nature of physical analogy concerned the two senses in which analogies are incomplete. This material was best presented in Maxwell's very last work, the *Elementary Treatise on Electricity*, published posthumously in 1881. It was there illustrated in terms of Kelvin's analogy between electrostatics and heat conduction, but here it will be simpler to translate the illustration to the analogy between electrostatics and fluid motion. First, analogies were found incomplete in the sense that the one science has only a special group of instances which are analogous to the other science.[3] As already described, the analogy between fluid motion and electrostatics holds only for the special group of instances in which the motion of the fluid is steady and in which the fluid is frictionless and incompressible. There are additional limitations. The memoir 'On Faraday's Lines of Force' also pointed out that the fluid must fill completely the space under consideration. If there are islands, then the velocity of the fluid at a point will not vary inversely as the square of the distance from that point to a source.[4] Further, a continuous supply of energy from outside the system is required to

[1] 'On Faraday's Lines of Force *Papers*, **I**, 187. See also Maxwell's letter to Kelvin, 13 September 1855, written during the preparation of the memoir : 'Now I have been planning and partly executing a system of propositions about lines of force &c which may be *afterwards* applied to Electricity, Heat or Magnetism or Galvanism, but which is in itself a collection of purely geometrical conceptions of lines, surfaces &c.' Here Maxwell referred to physical analogy as 'allegorical representation'. *Origins of Clerk Maxwell's Electric Ideas*, p. 17.

[2] For a description of these theories see E. T. Whittaker, *History*, pp. 29-31, 37-39.

[3] *Elementary Treatise*, § 65

[4] 'On Faraday's Lines of Force', *Papers*, **I**, 159

generate the fluid and drive it through the sponge to the sinks, while, as stated in the *Elementary Treatise on Electricity*, a system of charged bodies placed in a good insulator will remain electrified indefinitely.[1]

Second, the analogies were found incomplete in the sense that certain quantities describing one set of phenomena may represent, to use Maxwell's terminology, ' physical states ', while the analogous quantities describing the other set of phenomena are ' mere scientific concepts '.[2] If a quantity is connected to other effects which are independently defined, then it is a physical state ; if not, then it is a mere scientific concept. The velocity of the fluid at any point in the space surrounding the sources and sinks represents a physical state. A change in this velocity means a change in the distribution of sources and sinks. But it also means a change in other properties of the fluid which are connected to the velocity in determinate ways, for example, the temperature, the density, and the viscosity of the fluid. The electric force at a point in the space surrounding the charged bodies is a mere scientific concept. A change in the electric force means only a change in the distribution of the charged bodies themselves. The concept of a force at a point in space is simply a device which the mathematician employs to compute forces in certain complicated problems which arise in the study of the interaction of charged bodies.[3]

To put the distinction between electric force and velocity in a slightly different manner : the electric force at a point is defined as the force which would act upon a unit charge if placed there. No independent definition exists. The only way to determine this force is by an actual test charge or by a calculation from the distribution of charge producing the force. The velocity of the fluid at a point might be defined as the force which would act upon a small cork tied to a spring if placed there. Here independent definitions do exist. The velocity could be determined by an actual test with such a cork or by a calculation from the distribution of sources and sinks producing the velocity. But this velocity could also be calculated from other properties of fluid. For example, if the law connecting the temperature of fluid to its velocity were known, then the velocity of the fluid at a point could be calculated from its temperature at that point.

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[1] *Elementary Treatise*, § 65

[2] Ibid., § 66

[3] Maxwell made this distinction in terms of Kelvin's analogy. He compared temperature as a concept representing a physical state to electrostatic potential as a mere scientific concept. Elsewhere concerning the expressions in the electrostatic field for energy, tension, and pressure, Maxwell wrote : ' If the action of the system E_2 on E_1 does in reality take place by direct action at a distance, without the intervention of any medium, we must consider . . . (these) quantities as mere abbreviated forms for certain symbolical expressions, and as having no physical significance '. (*Electricity and Magnetism*), § 105

Incompleteness of analogy in the second sense is not the same as the distinction between physical analogy and physical hypothesis. It is logically possible to claim that a given quantity represents a physical state without making the further claim that some hypothetical substance constitutes the quantity in question. Fourier's *Théorie analytique de la chaleur,* for example, treats heat as an entity that travels, but nowhere names the vehicle. There is no hypothesis either to the effect that heat is the flow of the hypothetical substance called caloric or to the effect that heat is a mode of motion of hypothetical particles called molecules.

The third point which served to clarify the nature of physical analogy concerned the development of a new language suitable for the exact yet economical statement of different physical analogies. ' On Faraday's Lines of Force ' was written under the handicap of a language too vague for its precise ideas. The fluid picture in electrostatics was suggestive, but, since the analogy is incomplete, it was also misleading. Here for students of semantics is a good illustration of the ' tyranny of words ', a phrase Maxwell himself employed upon occasion.[1] A new language was needed to express the similarity between them. Vector analysis, and in particular, the analysis of vector fields, filled this need.[2] The development of vectorial methods first paralleled, then became part of, the development of electromagnetic theory. Hamilton, it will be remembered, published his *Lectures on Quaternions* in 1853, and P. G. Tait developed the various uses of the differential operator in the 1860s. Viewed in the light of Maxwell's later work, Faraday's employment of lines of force appear as a method of describing vector fields without the benefit of mathematical notation, and Maxwell's use of tubes of fluid motion appear as something halfway between Faraday's methods and vectorial methods. The analogy of the first memoir is easily expressed today. In electrostatics, the curl of the electric force equals zero ; in the motion of Maxwell's ideal fluid, the curl of the velocity also equals zero. In electrostatics, the divergence of the electric displacement depends upon the volume distribution of the positive and negative charges; in the motion of the fluid, the divergence of the velocity depends upon the distribution of the sources and sinks.

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[1] Letter to R. B. Litchfield, 1858, Campbell and Garnett, *Life*, p. 219.

[2] Sir Joseph Larmor makes the point here developed in his *Aether and Matter*, Cambridge, 1900, p. 75.

Maxwell himself made no systematic use of vectorial methods. However, in *Electricity and Magnetism*, published in 1873, he stated some of his results in a vectorial notation. And in a paper 'On the Mathematical Classification of Physical Quantities', published in 1871, he suggested some names, including 'curl', for the various operations with Hamilton's differential operator. Maxwell's names were designed to associate certain physical ideas with the mathematical operations.[1] The paper and also *Electricity and Magnetism* foresaw the extensive use of vectorial methods in the analysis of three-dimensional problems in physics. Maxwell regarded vector analysis as the natural language of electromagnetism and of such comparable disciplines as hydrodynamics and elasticity.

> The invention of the calculus of Quaternions is a step towards the knowledge of quantities related to space which can only be compared for its importance, with the invention of triple co-ordinates by Descartes. The ideas of this calculus as distinguished from its operations and symbols, are fitted to be of the greatest use in all parts of science.[2]

The paper on classification suggested a distinction between the physical classification of quantities and the mathematical classification. Maxwell believed that this distinction when fully developed would serve in the correct expression, even detection, of physical analogies.

> . . . if we had a true mathematical classification of quantities, we should be able at once to detect the analogy between any system of quantities presented to us and other systems of quantities in known sciences, so that we should lose no time in availing ourselves of the

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[1] 'On the Mathematical Classification of Physical Quantities,' *Proceedings of the London Mathematical Society*, **3**, No. 34; reprinted in *Papers*, **2**, 263-266

[2] Ibid., *Papers*, **2**, 259. See also the introductory section to *Electricity and Magnetism*, § 10. Maxwell writes: 'As the methods of Des Cartes are still the most familiar to students of science, and as they are really the most useful for purposes of calculation, we shall express all our results in the Cartesian form. I am convinced, however, that the introduction of the ideas, as distinguished from the operations and methods of Quaternions, will be of great use in the study of all parts of our subject, and especially electrodynamics, where we have to deal with a number of physical quantities, the relations of which to each other can be expressed far more simply by a few expressions of Hamilton's, than by ordinary equations!'

> mathematical labours of those who have already solved problems essentially the same.[1]

The physical classification of a quantity was to be according to the science in which the quantity occurred, while the mathematical classification was to be according to the form of the quantity. Thus temperature may be classified with pressure, for both occur in the science of thermodynamics, but temperature may also be classified with mass, for both are scalar in form. The paper pointed out such other forms of quantities as vectors referred to lines or line integrals, vectors referred to areas or surface integrals, and the different kinds of vector fields.[2]

2

In the above discussion of the nature or character of physical analogy, some mention was made of the uses of this method. Analogy, Maxwell stated in his address to the British Association, was '. . not only a legitimate product of science, but capable of generating science in its turn.' [3] In the course of his writings, Maxwell distinguished, in effect, two ways in which analogy generated science. One, the detection of an analogy made it possible to transfer the solution to a mathematical problem in one branch of physics to another branch of physics. Two, the attempt to make an analogy more complete served to direct entirely new theoretical and experimental inquiries. Maxwell also discussed the doubts sometimes felt as to whether analogy was a condition of scientific inquiry or just an expository device.

The use of physical analogy to transfer the solution to a problem from one branch of physics to another is illustrated by many of the advances in electricity and magnetism. The paper on classification stated, for example, that, for the Italian physicist Mossotti to obtain the mathematical theory of electrostatic induction, he had only to take Poisson's work on magnetic induction, and translate the magnetic language into electric and the French into Italian.[4] Maxwell's review of Kelvin's *Reprint of Papers on Electrostatics and Magnetism* remarked that Kelvin's analogy between electrostatics and heat conduction made the mathematical methods of Fourier available to electrostatics.[1] The analogy between electrostatics and fluid motion transferred problems concerning distributions of sources and sinks into problems concerning distributions of charged bodies. In his third great memoir on electricity, ' A Dynamical Theory of the Electromagnetic Field, ' published in 1864, Maxwell demonstrated that the equations of motion developed for the study of material systems by Lagrange, some hundred years earlier, in the *Mécanique analytique* were also applicable to electric circuits.[2]

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[1] 'On the Mathematical Classification of Physical Quantities,' *Proceedings of the London Mathematical Society*, **3**, No. 34, reprinted in *Papers*, **2**, 258

[2] Ibid., *Papers*, **2**, 257-262. See also *Electricity and Magnetism* §§ 11-19

[3] 'Address to the Mathematical and Physical Sections of the British Association', *Papers*, **2**, 227

[4] 'On the Mathematical Classification of Physical Quantities,' *Papers*, **2**, 258

The second use of physical analogy is more profound. Analogies are incomplete, and the attempt to make them more complete suggests new directions for inquiry. Further, physical analogies sometimes develop into acceptable physical hypotheses. Maxwell's creation of the mathematical theory of the field and the electromagnetic theory of light nicely illustrates how the method of analogy is, to use Maxwell's own term, ' science-forming '.[3] The memoir ' On Physical Lines of Force ' consciously employed two analogies. One analogy is between a stationary field and a solid under stress. Since the simplest medium which can support transverse wave-motion is an elastic solid, this analogy proved singularly fruitful. The other analogy is between electricity and fluid motion, with its suggestion that Ampère's law be modified to satisfy the equation of continuity. Maxwell's work in kinetic theory illustrates the development of a physical analogy into a physical hypothesis. Kinetic theory began in the root analogy between the observed properties of gases, and a system of small, hard, perfectly elastic spheres, acting upon one another only during impact.

Philosophers of science have made the claim that analogy, in the nineteenth century, did not so much direct inquiry as aid the exposition of discoveries obtained by other means. The comparisons to mechanics made the introduction of the new ideas easier by relating the unfamiliar to the known. But Maxwell, for his part, found it just as hard to believe that Ampère, who presented *his* results directly in mathematical terms, actually obtained these results in such fashion. *Electricity and Magnetism* charged Ampère with erecting a ' scaffolding ' only to remove it prior to publication.

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[1] Review of *Reprint of Papers on Electrostatics and Magnetism*, in *Nature*, **7** ; reprinted in *Papers*, **2**, 302

[2] ' A Dynamical Theory of the Electromagnetic Field,' *Royal Society Transactions*, 1864 ; reprinted in *Papers*, **1**, 526–597

[3] Review of *Reprint of Papers on Electrostatics and Magnetism*, *Papers*, **2**, 301

> The whole, theory and experiment, seems as if it had leaped, full grown and full armed, from the brain of the 'Newton of electricity'. It is perfect in form, and unassailable in accuracy, and it is summed up in a formula from which all the phenomena may be deduced, and which must always remain the cardinal formula of electrodynamics.
>
> The methods of Ampère, however, though cast into an inductive form, does not allow us to trace the formation of the ideas which guided it. We can scarcely believe that Ampère really discovered the law of action by means of the experiments which he describes. We are led to suspect, what, indeed, he tells us himself, that he discovered the law by some process which he has not shown us, and that when he afterwards built up a perfect demonstration he removed all traces of the scaffolding by which he had raised it.[1]

To be sure, Maxwell admitted that analogy also functioned as an aid to the understanding both for the original investigator and for the reader. At the time of publication of the first memoir fluid motion was more familiar to scientific minds than electricity. The *Elementary Treatise on Electricity* suggested that if the history of science had been different, then the various analogies exhibited by electrical phenomena '. . . would have served rather to obscure than enlighten minds'.[2] Further, Maxwell recognised the existence of different kinds of minds engaged in the scientific enterprise. In the address to the British Association he found that the laws of physics may be expressed either symbolically, as relations among abstract quantities, or as physical analogies.

> There are men who, when any relation or law, however complex, is put before them in a symbolical form, can grasp its full meaning as a relation among abstract quantities. Such men sometimes treat with indifference the further statement that quantities actually exist in nature which fulfil this relation. The mental image of the concrete reality seems rather to disturb than to assist their contemplations. . . .
>
> Others, again, are not content unless they can project their whole physical energies into the scene which they conjure up. They learn at what a rate the planets rush through space, and they experience a delightful feeling of exhilaration. They calculate the forces with which the heavenly bodies pull at one another, and they feel their own muscles straining with the effort.

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[1] *Electricity and Magnetism*, § 528. See also Maxwell's letter to Kelvin, Nov. 13, 1854, *Origins of Clerk Maxwell's Electric Ideas*, p. 8. The confession of Ampère to which Maxwell refers in the quotation above is in *Théorie des phénomènes électrodynamiques*, p. 9.

[2] *Elementary Treatise*, § 68

> To such men momentum, energy, mass are not mere abstract expressions of the results of scientific inquiry. They are words of power, which stir their souls like memories of childhood.
>
> For the sake of persons of these different types, scientific truth should be presented in different forms, and should be regarded as equally scientific, whether it appears in the robust form and the vivid colouring of a physical illustration, or in the tenuity and paleness of a symbolical expression.[1]

In fine, analogy is a condition of scientific speculation. It was *the* condition for Maxwell, though he was ready to allow other conditions as well.

3

Some of the now classic writers on the philosophy of science touched on the method of physical analogy. For the most part, however, they only raised again the points made by Maxwell himself. Pierre Duhem, in *La Théorie physique*, defined physical analogy as a similarity in mathematical form between two sciences which describe different sets of phenomena.[2] In *Les Théories electriques de J. Clerk Maxwell* he even cited 'On Faraday's Lines of Force' as a prime example of the correct and successful use of physical analogy.[3] Further, *La Théorie physique* distinguished, in effect, between physical analogy and physical hypothesis,[4] and discovered two types of minds engaged in science. These two types, for Duhem, followed national boundaries, with the broad but weak mind of the English reflected in a reliance upon illustration, and the narrow but strong mind of the continental scientist reflected in a taste for purely symbolical expression.[5] Ernst Mach, in *Erkenntnis und Irrtum,* also mentioned the analogy of the first memoir. He remarked upon the advantages of physical analogy, noted that analogies are incomplete, and illustrated some of these considerations using the analogy between light and small disturbances in an elastic solid.[6] Norman R. Campbell, in *Physics : The Elements,* discussed physical analogy and physical hypothesis which he called physical theory. For the physical theory of a given set of phenomena to be adequate, it must be more than a mathematical description. It must display an analogy to some other set of phenomena.[1] Campbell cited as examples of such physical theories the kinetic theory of gases and, in *What is Science?*[2] the electromagnetic theory of light.

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[1] 'Address to the Mathematical and Physical Sections of the British Association', *Papers*, **2**, 219-220

[2] Duhem, *La Théorie physique*, Paris, 1906, Part I, Chapter IV

[3] Pierre Duhem, *Les Théories electriques de J. Clerk Maxwell*, Paris, 1902, pp. 48-58

[4] Duhem, *La Théorie physique*, Part I, Chapter IV

[5] Ibid., p. 169

[6] Ernst Mach, *Erkenntnis und Irrtum*, Leipzig, 1905, pp. 217-228, 241

To be sure, there are important differences between Duhem and Mach, on the one hand, and Campbell, on the other. Duhem and Mach, in the tradition of Rankine and Ostwald, rejected physical hypotheses as a legitimate goal of physics. They favoured phenomenological theories. In contemporary terms, they wanted to restrict physics to macroscopic theories, eliminating microscopic theories. Upon occasion Maxwell himself was also prepared to discard physical hypotheses. For Maxwell, however, such rejection was not a matter of principle but of evidence. In the memoir 'On Physical Lines of Force', he offered a physical hypothesis for electromagnetic phenomena which was parallel in many respects to the kinetic theory of gases. Maxwell subsequently discarded this hypothesis but retained the kinetic theory.

By way of conclusion, it is well to distinguish between the method of physical analogy and the familiar argument by analogy. The argument by analogy as applied to science looks something like this. Some set of phenomena A is known to be like a set of phenomena B with respect to properties x, y, and z. And set of phenomena A, in addition, is known to possess property w. Therefore, so the argument runs, B must also possess w. The argument by analogy is not deductive; the conclusion does not necessarily follow from the premisses. There is a certain probability, however, that B also possesses w. The measurement of this probability is investigated in inductive logic.

The method of physical analogy differs in an important way from the argument by analogy. Again the set of phenomena A is like the set of phenomena B with respect to properties x, y, and z. And again A, in addition, is known to possess w. Now, does B also possess w? The question comes from the analogy, but the answer must come from further theoretical and experimental investigation. It is not a matter of arguing from old premisses, but of establishing new ones. Success consists either in finding evidence that B does possess w or that B does not possess w. In the first case, the analogy is made more complete; in the second case, the exact limits of the analogy are discovered. Both procedures are 'science-forming', both are parts of the method of physical analogy.

[1] Norman R. Campbell, *Physics: The Elements*, Cambridge, 1920, pp. 120–140

[2] Norman R. Campbell, *What is Science?* London, 1921, Chapter V

NOTES AND COMMENTS

A Discussion of Professor Kapp's Views

Professor Kapp's interesting lecture [1] contains much with which I can fully agree. The points on which I differ from him will, however, form the bulk of this note, since there is little advantage in repeating what has already been clearly stated.

The following points seem to be particularly significant :

(i) Kapp states in *A*3 that randomness of creation is implicit in any theory of continual creation. This does not seem to be a necessary connection since it is conceivable that a particular condition (say, of a gravitational nature) causes the creation of particles without necessarily implying the conservation of energy. Random creation seems by far the most plausible way of formulating a theory of continual creation, but the connection is not necessary.

(ii) The arguments given against the hypothesis of continuous existence in the past are not completely convincing. In particular Tolman's relativistic thermodynamics [2] seems to offer a way out of Kapp's point (1) (of which point (2) is a direct consequence). The relativistic argument at the beginning of point (3) is incorrectly stated and again does not constitute logical proof.

Kapp's arguments, it should be pointed out, are strong and make his conclusion highly plausible, but cannot be said to prove it beyond doubt.

(iii) Kapp's arguments against disappearance by removal would seem to be quite erroneous, but the point is rather recondite.

In any expanding model of the universe the problem of the ' horizon ' arises. Expansion, incidentally, follows directly from the observed red shift of the galaxies unless a special hypothesis is made to account for the red shift, a procedure that Kapp has cogently and clearly criticised.

Observationally the recession velocity is proportional to distance. In all theories there is a more or less complicated functional dependence that for small distances (i.e. the observed range) approximates to proportionality. However, at large distances the functional dependence, though differing in the various models, is always such as to ensure that no object is ever [3] accelerated to the velocity of light or beyond it. This applies to all relativistic models and to the model of the steady-state theory although contrary statements have occasionally been made. In none of these theories does any object that at any time was observable to an observer, and hence moving at less than the speed of light relative to him, ever in the reckoning of this observer appear to him to reach or exceed the speed of light and so become unobservable.[1] In some relativistic models, however, the opposite process takes place, viz. a previously unobservable object becomes observable at some instant.

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[1] See pp. 177-185 of this issue.

[2] R. Tolman, *Relativity, Thermodynamics and Cosmology*, Oxford 1934

[3] In the precise sense defined in the sentence after the next.

The horizon, as the limit of the observable universe is called, has particularly simple features in the steady-state model. In discussions of this type, it must be remembered that the physicist is primarily concerned not with the absolute horizon (the limit of the observable region as seen by a telescope of infinite power) but with the horizons corresponding to instruments of arbitrarily large but finite power. In this manner Professor Kapp's excellent arguments against the use of infinity are faithfully followed. In the steady-state model any instrument of finite power always sees a finite number of galaxies, a number that is constant in time except for statistical fluctuations. Any given galaxy first became visible at the time of its formation when it began to shine. Owing to the recession its apparent intensity will eventually diminish more and more until it is no longer observable with the particular instrument in question. It never ceases to be visible in principle, i.e. at any time a sufficiently powerful telescope will enable us to see it.

It will be noticed that this manner of disappearance by removal does not involve the concept of infinity in any way but deals instead with finite numbers, finite instruments, and finite times. Moreover, this disappearance by removal is an almost direct consequence of the straightforward interpretation of the observations of the red shift as recession.

(iv) The problem of very old centres of attraction in the steady-state model is highly interesting. There are in fact good reasons for believing that such clusters of galaxies do not grow beyond a certain size owing to the operation of perfectly ordinary physical laws (temperature effect).[2] Even if this were not so, the extreme infrequency of very old objects is so great in the steady-state model that their spheres of influence, on any plausible hypothesis of growth, cover only a negligible fraction of space.

(v) It is not quite easy to see what Professor Kapp's views on disappearance by removal are. If the direct interpretation of the red shifts as velocities of recession is accepted (as Kapp does) then there is *some* disappearance (from the range of an instrument of finite power) by removal. If there is in addition disappearance by extinction a highly complicated world model results, in which (if steady conditions are maintained) the rate of continual creation is balanced by the sum of the rates of disappearance by removal and by extinction. Such a model seems to be unnecessarily complex and offers few, if any, compensating features.

[1] F. A. E. Pirani, *The Observatory*, 1954, 74, 172

[2] D.W. Sciama (to be published shortly)

A fairly simple and interesting model arises if one combines disappearance by extinction with continual creation in a *static* model. In such a model a special hypothesis is required to account for the red shifts, but a symmetrical law of creation and extinction might be considered an attractive feature.[1] It is interesting to examine the observable difference between such a static model and the steady-state expanding model. Both models satisfy the perfect cosmological principle which states that, by and large, the universe presents the same aspects from all places at all times. Accordingly both these models differ from all other models (in which the universe evolves) in that the observable properties of distant and near galaxies should be identical. The two models differ from each other, however, in the age distribution of the galaxies. In the steady-state theory the evolution of galaxies must exactly counterbalance the expansion of the system. Since this is exponential the age distribution must be exponential, with a large preponderance of young galaxies. On the static theory, however, the ages of galaxies should be grouped round a well defined average, as explained at the end of Professor Kapp's lecture.

The static model with creation and extinction is valuable in clarifying cosmological concepts. At the present time, however, it is doubtful whether there is justification for the substitution of extinction for, or its addition to, a process of removal that follows by a simple inference from the observed red shifts of the spectra.

H. Bondi

A Reply to Professor Bondi

It is reassuring to me that Professor Bondi and I can find so much of substance on which to agree. I gladly accept the Editor's invitation to comment on the points on which we differ, for it is from discussion of these that one may hope for the most fruitful result. I use below the numbers as they occur in Professor Bondi's notes :

(i) I doubt if the point would have arisen if the word ' origin ' were always used instead of ' creation '. I agree with Bondi that one could just conceivably regard some particular condition in the existing universe as the cause and creator of new particles. But I think it is as essential to Bondi's view as it is to mine to avoid the assumption of either cause or creator. Incidentally it may not be generally realised that neither of us implies a failure of the law of conservation of energy. Correctly formulated this law applies only to a self-contained system. The hypothesis of continuous origin implies that the whole universe is not, in the sense implied by the law, a self-contained system.

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[1] On the other hand an unsymmetrical law may be considered necessary to account for the time-asymmetry of our surroundings.

(ii) I agree.

(iii) Bondi's point is important and very interesting. I admit that I had not appreciated it before. But I do not think that it has any bearing on the question whether the continuous disappearance of matter is by removal or by extinction. The point is that in an expanding universe any star that is now observable and appears, therefore, within the optical horizon will never cease to be a source of signals that reach the earth ; it will only grow redder and fainter. The reason is because the time interval between the emission and the arrival of a signal increases as the star recedes, and the increase of this interval is at such a rate that the signal that reaches the earth is always emitted *before* the speed at which the star recedes has reached the velocity of light.

I agree that therefore in a place like the earth, from which an infinitely massive star never *has* been observed, one never *will* be observed. Our field of observation does and always must contain only finite physical quantities. But the Bondi model does nevertheless represent infinite quantities : infinite radius, an infinite amount of mass, and, inescapably I think, some infinitely intense gravitational fields. To save the model one must either be satisfied that it is legitimate to apply the concept infinity to physical realities or one must treat the words ' unobservable ' and ' non-existent ' as synonyms.

(iv) My objection would still hold, of course, even if one could prove that after a centre of gravitational attraction has attained a limiting size, it ceases to have a gravitational field or is prevented in some other way from becoming infinitely massive.

(v) Perhaps I ought to make it clear that my model is one of finite but variable size. The rate of random origin for the whole may sometimes be greater and sometimes less than the rate of random disappearance. So the whole universe may sometimes happen to expand and sometimes to contract. In such a model some stars may at times become nearly and at other times quite unobservable, but that does not constitute disappearance by removal in the sense in which the Bondi model implies it.

So much for the more philosophical considerations. May I take this opportunity to mention a possible observational means of testing the hypothesis of removal by extinction. According to this hypothesis a small star remote from any others should capture more new matter by virtue of its gravitational field than it lost by extinction and would grow. But a small star near one or more large ones would not capture so much. Some particles that would fall on the small star if it were isolated would fall on the large stars instead. The small star would then dwindle and eventually vanish. This would happen in particular to planets and their satellites. So one should expect, according to disappearance by extinction, that the earth was becoming less massive. If it is so, its hold on the moon must be weakening. It may be possible to check this. The question to be investigated is whether the distance of the moon from the earth, and that of other satellites from their planets, is equal to or greater than the distance that can be accounted for by tidal action alone.

R. O. KAPP

Remarks upon Professor Bondi's Views

I PROPOSE to discuss two aspects of this subject, on one of which I find myself entirely in agreement with Professor Bondi, and on the other of which I am in disagreement.

The first is concerned with the status of a logical dichotomy into a proposition and its complement. To say that Socrates is pink or not pink is not to make an assumption about Socrates. We do not *assume* that Socrates is pink or not pink. We *know* that Socrates is pink or not pink. We thus *know* that the universe is finite or not finite in space, finite or not finite in time, in thermodynamic equilibrium or not, with a background illumination that is finite or not finite, and finally that the law of the inverse square is true or false. None of these logical alternatives involves any assumption about the universe. They would be equally true of a piece of cheese. Logical conjunction of n dichotomies of this kind involves 2^n disjunctions which could be displayed in a (2×2) contingency table in n dimensions. A typical entry in such a table would read 'infinite in space, finite in time, not in thermodynamic equilibrium, with a finite background of illumination, the law of the inverse square being uniformly true in all times and places'. Each member of this catalogue of conjunctions can be examined for compatibility with the general and statistical laws of mechanics on the one hand, and particular facts of observation, such as the intensity of background illumination, on the other hand. In the process a large number of members of the catalogue get struck out. I am in complete sympathy with this method of procedure but would like to point out that it involves no process whatever of *a priori* reasoning. Its application to the universe results in striking out one set of propositions; applied to a piece of cheese, a different set of propositions would get struck out because the relevant observations in terms of which the examination would be made differ in the two cases.

My second point is concerned with the status of the perfect cosmological principle. We accept something as a principle if we feel it to be in some sense absolutely true. A principle cannot be approximately true and if a statement is approximately true it cannot have the status of a principle, though it may be an acceptable description of a fact. The existence of asymmetrical objects, fluctuations in density and unique historical events, makes it clear that the Universe, in detail, is not homogeneous in space and time. Macroscopically, however, the latter proposition may be statistically true. If so, it has the status of an approximately correct interpretation of the evidence. It could only be raised to the status of a 'principle' if some definition of 'approximately' could be incorporated in it as an integral part. This would, I suppose, have to take the form of expressing the standard deviations involved in terms of one of the fundamental constants of nature, just as Heisenberg incorporated *h* into the indeterminacy principle. So far as I know Professor Bondi has not yet succeeded in doing this and therefore we cannot accord what he calls 'the perfect cosmological principle' the status he claims for it. It must therefore remain as a permissible generalisation of the evidence until he can do so.

Even should he succeed in incorporating a statistical measure of dispersion into his argument, it would be necessary to have observational evidence against the explosion theory before raising his proposal beyond the level of a hypothesis. I do not think these semantic points are by any means trivial. This subject is a difficult one and attention to verbal niceties is surely helpful. A description has one status, a hypothesis has another, and a principle has a third. They are not synonymous and should not be treated as such.

HALSBURY

Comment on Lord Halsbury's Remarks

LORD HALSBURY'S second point arises from a different usage of the term 'principle' in ordinary life and in certain branches of physics. It was Einstein who introduced the terms 'Mach's principle' and 'cosmological principle' some forty years ago to denote basic assumptions of great influence without wishing to indicate the likelihood of their correctness. We would not have been justified in introducing a new term after thirty years' general use of the term 'cosmological principle'.

I have frequently stressed the need for a precise statement of the cosmological principle. A limiting formulation would seem to be the most promising possibility.

As for the status of the steady-state theory, I would claim only this: As the steady-state theory is more vulnerable (and also intrinsically simpler) than the explosion theory, observational efforts should be made (and in fact are being made) to check the predictions of the steady-state theory.

H. BONDI

A Logical Basis for Genetics?

WOODGER[1] wishes to substitute for the term 'an inborn character' the allegedly more precise term 'an environmentally insensitive set of lives'. I certainly do not defend the term 'inborn character', and try to avoid using it. But Woodger's definitions do not appear to have reached precision. In particular they have not reached the kind of precision which geneticists need.

He uses the expression

$$\overline{Dlz}\,(X, Y, P),$$

which means 'every X which develops in a Y does so into a P'. X is a set of zygotes, Y of environments, and P of 'lives'. It might have been better to say 'Every member of X which develops in a member of Y develops into a member of P'. But I have quoted Woodger verbatim. Notice the word 'every'. If the universe of discourse are the flies which emerged in Professor Haldane's laboratory in 1952, such statements are trivial. But I assume that they are intended to be applicable to future events, for example that X could be 'all *Drosophila melanogaster* females of the same range of genotypes as the specimens in bottle A', that Y could be 'all bottles holding 500-600 ml. kept between 20° and 25° in which the amount of no constituent deviates by more than 5 per cent from the amount present in bottle B', and so on.

He goes on to define $X = \overline{Z}(P)$, 'X is the zygotic range of P' when 'only from a member of X . . . do you get a P', whatever be the environment. Unless this is a statement about what happened in a given laboratory in a given year, or something of the kind, it is a universal statement. I believe it to be absolutely meaningless in many cases, and probably in all cases. By interference with the development of stocks whose members do not normally develop into P, it is often possible to obtain a P. This is sometimes done by crude methods such as temperature shocks, sometimes by methods which allow of more insight into the process.

Let us take a particular case. P is the set of all imaginal *Drosophila melanogaster* which lack yellow ommatin in their eyes. This ommatin has been shown to be a derivative of tryptophan. Their eyes are scarlet if red pigment is present, otherwise they are white. Provided this insect is fed on 'normal' food, e.g. food where its main protein source consists of living cells, only pupae homozygous (or hemizygous) for at least one of what is believed to be a finite set of recessive genes fail to develop yellow pigment and produce flies belonging to P. These genes have been given names such as 'scarlet' and 'vermilion'. But a Drosophila belonging to a stock which normally develops this yellow pigment fails to do so when fed on a diet in which tryptophan is sufficiently short. On such a diet many larvae die, but those which develop into imagines have eyes without appreciable amounts of yellow pigment, indistinguishable from the eyes of 'vermilion' flies. Thus the zygotic range of P is the entire species.

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[1] J. H. Woodger, 'What do we Mean by "Inborn"?', this *Journal*, 1953, 3, 319-326

Conversely, larvae of stocks which do not normally develop yellow pigment can often be induced to do so by injecting intermediates between tryptophan and the yellow pigment, such as kynurenine, or by implanting organs from normal flies. Thus the zygotic range of $\bar{P}$ (not P) consists of most of the species, though our present techniques will not induce its development by flies of certain genotypes.

Other similar examples could be given. It is true that at present we do not know how to make a larva of a wingless stock develop wings. It is the working hypothesis of physiological genetics that we cannot yet do this because we do not know enough developmental physiology, and for no other reason. This hypothesis may be wrong. But every year such hypotheses are proved to be right for a few more cases.

That is to say, Woodger's basic definitions are certainly inapplicable to some cases, and may be inapplicable to any case whatever.

Woodger then goes on to define a set P as 'environmentally insensitive' if every member of the set of zygotes $X = \bar{Z}(P)$ develops into a P provided it reaches a certain chronological age or developmental stage. This is not a definition; it is a defiance. Woodger defies any developmental physiologist to make a member of a certain set of zygotes develop into anything but a P. To take one of his own examples, he challenges any physiological botanist to induce a plant to grow green leaves if it belongs to a stock which, in any environment where it has grown leaves at all, has so far grown yellow leaves. I think it highly probable in view of recent work on the biosynthesis of chlorophyll, that this challenge will be successfully taken up.

In a concluding footnote he questions the value of a logic for genetics based on differences rather than characters. He is, in fact, still an Aristotelian. He finds it easiest, as we all do, to think in terms such as 'every member of X is a member of Y', which is a slightly more precise way of saying 'every A is a B'. Aristotle developed his logic largely as the result of his difficulties in classifying animals. It worked very well for two thousand years.

So far as biology is concerned, Darwin pronounced sentence on Aristotelian methods when he wrote 'In short, we shall have to treat species in the same manner as those naturalists treat genera, who admit that genera are merely artificial combinations made for convenience'. To quote another passage from the same paragraph of the last chapter of the *Origin of Species*, 'we shall be led to weigh more carefully and to value higher the actual amount of difference between them'. A Darwinian logic will be based on differences, as the Aristotelian logic was based on similarities.

Woodger dislikes this prospect because ' neither is the difference between a red-eyed and a white-eyed Drosophila a Drosophila or an eye '. Nor is the minimal distance between a point and a surface a point or a surface. Nor is the difference between two musical notes a note or a chord. It is an interval, and without the notion of an interval musical theory would be impossible.

The notion of a difference, however, at once makes genetics a science rather than a set of anecdotes about class membership or set inclusion. The difference between a red-eyed [1] and a scarlet-eyed Drosophila is the same as the difference between a yellow-eyed and a white-eyed one, namely the difference between the presence and absence of a particular yellow pigment. It would not be grossly inaccurate to say that the difference was the pigment. This difference may be due to any of several gene substitutions, to a difference in the diet, or to other causes.

However, I quite agree with Woodger that such terms as ' innate character' and 'acquired character ' no longer serve a useful purpose, if they ever did so. Unfortunately at the present moment they are acquiring a new lease of life under slightly different names. Students of animal behaviour are disputing as to whether certain activities are ' instinctive ' or ' learned '. I do not think that there would be any advantage in substituting ' environmentally insensitive ' for such ill-defined terms as ' an unconditioned reflex ' or ' an instinct '. Let us consider such an activity as mammalian breathing. It is probable that no mammal has ever reached the age of even one day without breathing, that is to say filling and emptying the lungs by muscular contractions. If so mammalian breathing has so far been an ' environmentally insensitive activity '. But as a man can live for a day without breathing, in a chamber where the air pressure is changed by 20 per cent or so six times per minute, there is little doubt that a new-born mammal could do so.

To conclude, precision in biological terminology is desirable. And Woodger's search for it is laudable. But I contend that it is better to continue to use admittedly imprecise terms such as ' inborn character ', or even ' species ' rather than to replace them by other terms which can be shown to be equally imprecise. A century ago physicists wrote of ' permanent gases ', meaning gases which had *not yet* been liquefied. Similarly in my opinion an environmentally insensitive set *P* merely means one from which a genetically qualified individual, in Woodger's words a member of the zygotic range of *P*, has *not yet* been excluded by a suitable change in its environment, provided that it lived long enough. A zygotically insensitive set similarly means a set such that no genotype has *yet* been found which excludes development into a member of this set in a suitable environment.

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[1] The eye of the wild type is generally called ' red '.

A ' permanent gas ' would have been a good guide to scientific prediction had it not been a challenge to such men as Dewar, who liquefied a number of such gases. An ' inborn character ' or an ' environmentally insensitive set' would be an equally good guide if developmental physiologists did not exist.

Dr Woodger can of course ask me for better definitions, and I can only reply that I am at present more interested in experimental work. I should be very glad to have a system of definitions in which I could not pick holes, but I consider that the adoption of a system which (in my opinion) is demonstrably imprecise would only retard the progress of genetics, and that the use of such a system by certain geneticists was largely responsible for the drastic and (in my opinion) unjustifiable criticisms to which Mendelian genetics have been subjected in the Soviet Union.

J. B. S. Haldane

Some Comments on ' The Age of the Universe '

May I offer some critical comments on Mr M. Scriven's prize-winning article on this subject? [1] My purpose is to reinforce Scriven's very plausible argument by eradicating what appear to be four confusions that arise in the course of its development.

Scriven claims that one of the difficulties presented by the question ' What is the age of the Universe? ' arises from the fact that its significance depends upon physical theories about the nature of matter. The problem is stated in the following way.[2]

Since ' the Universe ' means ' everything that exists ', then the phrase ' the origin of the Universe ' will be equivalent to ' the origin of the first thing '; but, asks Scriven, what exactly is to count as a thing? Suppose we agree that photons and electrons are to count as things, and discover the first moment that any of them existed. This would constitute the temporal origin of the Universe only on the supposition that this theory of the structure of matter is the correct one. If later we were to discover certain short-lived particles whose decay produces electrons, it would be necessary, he holds, to try to date their origin in the pre-electron time record.

Scriven asserts that this problem about the nature of the fundamental substance raises a serious difficulty if we attempt to show that the Universe began at a certain time, because it is only in so far as we can give a sense to the statement ' Science has now identified the fundamental substance ' that we can give a sense to the statement ' There was a definite moment when this substance originated ', i.e. ' The Universe had an origin in time ' (p. 186).

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[1] This *Journal*, November, 1954, pp. 181 ff.

[2] Ibid., p. 186

I suggest that Scriven's difficulty here is an illusory one, because the problem of the origin of the Universe can be stated without mentioning, or even presupposing, anything concerning the fundamental substance. Can we not translate ' the date of the origin of the Universe ' into ' the date of the first event '? Both the question and its translation are, however, ambiguous, though not, I hope to show, fatally so. There is no logical absurdity in the conception of a series of non-physical events antedating the first event of the physical series, though it is doubtless impossible to give this notion any empirical meaning by means of the principle of verifiability; in any case it is not an issue that can arise for natural science which is exclusively concerned with physical processes. It appears, then, that Scriven's explanation that ' the Universe ' means simply ' everything that exists ' is too vague, and that a more accurate translation would be ' every physical event (or thing) that exists or existed '. But does not Scriven's difficulty about the fundamental substance arise in connection with physical events as well as physical things? If so, then my proposed reformulation of the problem of cosmogony achieves nothing. It seems quite clear, however, that the definition of ' physical event ' does not presuppose any theory about the essential nature of matter, for this definition would specify only the general properties common to all physical events of whatever magnitude, for example that they can be located within some system of three-dimensional spatial co-ordinates. There is no logical connection between the concept of a physical event and hypotheses concerning the fundamental substance, so that Scriven's puzzle is removed and replaced by the more tractable problem of clarifying the criteria by reference to which physical and non-physical events are distinguished.

Scriven fails to consider this sort of interpretation of the problem because he throughout assumes that the question of the origin of the Universe can be stated only in terms of the concept of substance, a surprising view to hold these days. For instance, he says in another context that unless we can give a meaning to the notion of time without things, we can give none to the time of the first things (p. 189). Here we should recall Whitehead's suggestion that the problems of physical theory are clarified if expressed in terms of events rather than things.

My other comments arise from the conclusion that Scriven draws from an interesting discussion of the meaning of the expression ' Nothing exists at time *t* '. His account of this may be summarised as follows. We are inclined to say that the sentence has a meaning because we can imagine the number of things in the Universe vanishing until there is only one left, when it too vanishes. The problem thus becomes, what is the meaning of ' The last thing has vanished '? This appears to make sense because there is a conceivable experience of a hypothetical observer that would be so described. Nevertheless, says Scriven, it is logically impossible to verify the proposition; he does not explain the nature of this logical impossibility, but presumably it arises from the fact that the observer himself would count as one object in the Universe. I shall say something more about this later. Scriven concludes this discussion in the following words:

> It is not possible to push this argument to an absolute conclusion, though consideration of a number of these cases makes us less and less willing to say that there is any meaning in ' Nothing exists '. This is not to be taken to mean that ' Something exists ' is *necessary*, i.e. that the Universe *necessarily* exists. The idea of necessary existence . . . is equivalent to the idea of deductive proof of future occurrences, and is in an important sense inconsistent with our logic (p. 189).

This passage raises three points. I shall state the least important first.

(1) The suggestion that the idea of necessary existence is connected with a ' deductive proof of future occurrences ' is not correct. In metaphysics, as distinct from physics, the concept of necessary being has no reference at all to time. In this context ' First Cause ' does not mean, as some have thought, an event that took place a very long time ago.

(2) Scriven's investigation of the logical status of ' Nothing exists ' is beside the point, which is ' What is the meaning of " Nothing exist*ed* " '? This alteration in tense makes a difference; not everything that can be said of the former sentence can be said of the latter. For example, although ' Nothing exists ' is not formally self-contradictory, it is one of those interesting sentences whose meanings are incompatible with the conditions under which they could be used to make a statement. In this case both the speaker and the symbols that he uses must exist if the assertion ' Nothing exists ' is to be made. A similar pragmatic paradox arises from any statement made by means of the sentence ' The last thing has vanished '. It appears that Scriven failed to identify the sort of queerness possessed by this sentence. It is odd not because it makes a claim that is logically impossible to verify, but rather because it can never make a claim at all, for what the sentence conveys is incompatible with any serious employment of it.

(3) Scriven suggests that the expression ' Nothing exists ' is meaningless. If this is so, then its contradictory, ' Something exists ' will also be meaningless. But clearly this is not the view that Scriven wishes to maintain; his denial that this proposition is a necessary truth indicates that he regards it as a contingent (and true) proposition. On this supposition the sentence ' Nothing exists ' will not be meaningless but will express a contingent though false proposition, for the contradictory of a contingent proposition will itself be contingent. It is therefore a confusion of logical categories to argue, as Scriven appears to do, that ' Nothing exists ' is meaningless while its contradictory ' Something exists ' is contingently true.

I have here followed the generally accepted assumption that the negation of a meaningless proposition is itself meaningless. Philosophers take this for granted, and indeed it seems self-evident that if it is nonsense to assert so-and-so it must also be nonsense to deny it. Nevertheless I am not certain that the practice of philosophers is always in accord with the principle; Scriven is not alone in being tempted to hold that although p is meaningless, not-p can be contingently true. Anyone who believes that ' God exists ' is meaningless is committed, by the principle just stated, to the view that ' God does not exist ' is also meaningless. Yet there are positivistically minded persons who maintain the meaninglessness of ' God exists ' and who are also convinced of the truth of the contingent proposition ' God does not exist ', though they may not be prepared to defend this latter assertion in a philosophical arena. It seems from this that the principle of the meaninglessness of the negation, although self-evident, is perhaps not true. In any event there is no doubt that it is not yet clear what follows from saying that a certain sentence is without meaning—because, of course, it is not altogether clear what it is to say that a given sentence does have a meaning. Certainly we could not expect Scriven to deal with these complex logical problems in addition to those of cosmogony.

C. K. Grant

University of Nottingham

Achilles and Dimensions

Professor L. J. Russell, in his review (this *Journal*, February 1955) of Gilbert Ryle's *Dilemmas* expresses dissatisfaction with his explanation of the paradox of Achilles and the Tortoise. As this explanation is the same as the one which I suggested some years ago (*Science: its Method and its Philosophy*, 1950), I feel I must point out that Professor Russell is not right in saying ' that when Achilles gets to the place from which the tortoise started, all he has done is to put himself in precisely the same position as he was at the beginning of the race, though the racecourse is now smaller, and this situation apparently gets repeated indefinitely '. Achilles is nearer the tortoise than at the start, and so he is not ' precisely in the same position '; and further, the verbal confusion over *never* is only transferred by Professor Russell to the words ' repeated indefinitely '. We can imagine the situations arrived at, repeated indefinitely, but the time intervals involved in the motion (*not* the acts of imagination) get smaller and smaller, and do not add up to an ' indefinitely ' large time. Surely this is a purely verbal problem and might now be laid to rest in peace.

Touching, now, the thorny and perennial question of dimensions: this subject is also not free from verbal troubles, including the astonishing fact that the word ' dimension ' was rarely precisely defined. Dr Whitrow, in his review (in the same issue) of C. M. Focken's new book on dimensional methods, records that Dingle followed Bridgman in insisting ‘ that dimensions characterise magnitudes, i.e. the results of measurements of physical magnitudes ’ (physical *quantities* surely?) ‘ and are not to be regarded as intrinsic properties of physical objects ’, and that this ‘ was one of the most important consequences of the theory of relativity ’. Bridgman and Dingle, however, define ‘ dimension ’ to mean the index or power to which each fundamental magnitude occurs in a physical equation. This makes the statement that the dimensions of time ‘ are simply [T] ’ (*Phil. Mag.*, 1942, **33**, 321) obscure, since this symbol is not an index.

I cannot help thinking that my definition (*Proc. Phys. Soc.* 1941, **53**, 418) which includes the symbol for the operation of measurement (e.g. [T]), *as well as the index*, is preferable. It helps to emphasise that the numbers obtained are the results of measuring operations, and not ‘ intrinsic properties of physical objects ’ (whatever this may mean). I was led to this, not by the theory of relativity, but by a statement of Eddington's, viz. the electric charge e is only manifested in the presence of other charges, and in particular, for simplicity, in the presence of *one* other charge, and it is a measure of the mutual interaction. We are apt to think of charge as something we rub off our sleeve with a fountain pen, and this, no doubt, is physically correct, but $e = 4{\cdot}80 \times 10^{10}$ E.S.U., is a statement of *metrical* physics which is only concerned with numbers obtained by specified measuring operations, and was never rubbed off any sleeve. As mass and charge can be measured by similar operations (e.g. Coulomb balance), I suggested that they should have the same dimensions. This led to a great simplification of dimensional formulae, and also, for instance, to $E = Mc^2$.

G. Burniston Brown

From Classical to Modern Chemistry. By A. J. Berry.
Cambridge University Press, 1954. Pp. xii + 251. 25s.

Mr Berry's book is a sequel to his *Modern Chemistry* (1946). The two books taken together make an extremely good short history of modern chemistry.

From Classical to Modern Chemistry is composed in an ingenious and novel way. The history of each of seven major chemical topics is briefly traced from the early nineteenth or eighteenth century (earlier in some cases), and then developed up to about 1935 much more fully. The outcome is a most stimulating study of nineteenth century chemistry, correctly related to earlier work and evaluated in the light of recent advances. Thus in Chapter IV, entitled *Physical Optics and Chemistry*, the discussion includes a remarkable quotation from Newton who described the diamond as 'probably an unctuous Substance coagulated' (p. 86), extends as far as the work of Hantzsch on indicators and nitric acid, and includes a brief account of the Raman effect. In this chapter, and those dealing with Heat, Electricity and Electrolysis, and Molecular Magnitudes, the author includes sections on the history of relevant physics. Throughout, the matter is presented comparatively and critically. For example, in his chapter on Kinetic Chemistry, Mr Berry puts in close juxtaposition the contradictory theories of catalysis held by H. E. Armstrong and W. Ostwald (p. 215). According to Armstrong pure substances are chemically unreactive and always need a catalyst if chemical change is to occur ; whereas Ostwald considered that a catalyst can never inaugurate a chemical change. Again, in the chapter entitled Analytical Chemistry, which is full of information difficult to find elsewhere, Berzelius' first value for the atomic mass of boron, 11·08, is compared with a modern value, 10·83 (p. 132)—a tribute to the analytical skill of Berzelius. Such examples greatly heighten the interest of Mr Berry's story, enabling him to combine a very considerable compression of style with readableness. Indeed, the book is one very good answer to the problem of *how* to write the history of science.

Between *Modern Chemistry* and *From Classical to Modern Chemistry* there is some overlapping which is fully justified by the author's plan. Both books refer to Victor Meyer and the non-existence of *two* isomeric methylene dichlorides. Both contain chapters devoted chiefly to classical molecular and atomic theory, but the new book has a much fuller account of the work of Werner and others on inorganic coordination compounds (pp. 178-190). The earlier book contains a relatively fuller account of the stereochemistry of organic molecules ; the later one does not mention the

Walden inversion. Some subjects are omitted from both books, but it is surprising how far the two books do in fact give a just account of modern chemical thought.

From Classical to Modern Chemistry contains no philosophy, with the exception of an occasional comment which shows that the author is a realist of the more or less uncritical kind (p. 122). Students of the philosophy of chemistry will need the book as raw material—any chemist will delight in it.

The book is well supplied with indexes and references. The date 1871 (p. 52) should be 1781. *Dufay* (p. 47) is usually spelt without a final *e*.

John Bradley

Sovereign Reason. By Ernest Nagel
The Free Press, Glencoe, Illinois, 1954. Pp. 315. $5.00

This is a collection of reviews and studies reprinted from various periodicals, written between 1930 and 1954. All except one are, he says, 'critical studies of contemporary philosophers who have been occupied, in one way or another, with the content or method of modern science, and who have thereby sought to illuminate the nature and operations of human reason' (p. 9). The writers studied include Peirce, Dewey, Whitehead, Russell, Eddington, and Reichenbach. The articles vary in importance, from brief notices of books (Nos. 8, 12, 13) to studies such as No. 5 on Pierce's *Empiricism*, Nos. 6 and 7 on Dewey, and No. 14 on Reichenbach's *Theory of Probability*. No. 10 is a reprint of Nagel's own article in the Russell volume of *Living Philosophers* with which readers in this country will be familiar.

The papers are written with Nagel's usual clarity and vigour, and he is scrupulously fair to his authors, however trenchant his criticisms may be. What he pleads for throughout is an attempt to understand the function of reason by an analysis of 'the procedures and conclusions of controlled enquiry' (p. 268) as practised by scientists, which he complains has been unduly neglected by most of the writers above, even including Dewey. He heartily approves of Peirce's statement that '. . . energy being a term in a dynamical equation, if we know how to apply that equation, we know thereby what energy is' (p. 93). We should shelve the term 'meaning' in philosophical discussion, and ask instead how terms are used (p. 94). Instead of asking what scientific terms 'designate' we should 'construe' them 'in terms of the procedures associated with them in specific contexts' (p. 96). This is a familiar view; the philosophy underlying it is described as 'contextualistic naturalism', and is well brought out in the short but admirable paper on *The American Temper* (No. 3), in the brief description (pp. 119–120) of the new intellectual atmosphere of the twentieth century which Dewey helped so greatly to bring about, and in the final paper, on *The Perspectives of Science and the Prospects of Men* (No. 16).

The title *Sovereign Reason* is somewhat intriguing. It is the title of the paper (No. 15) in which he examines the ' canons of rationality implicit in . . . philosophical idealism ' as expressed in the writings of Professor Bland Blanchard, concluding that the ideal of reason there advocated is ' false and irrelevant ', one which ' cannot permanently serve to guide the energies of sober men '. The phrase ' sovereign reason ' itself Nagel takes from a sentence (which serves as the motto for this paper) in Crawley's translation of Thucydides (IV. 108) in which men are said to ' use sovereign reason to thrust aside what they do not fancy '.[1]

As thus used by Nagel, the phrase is obviously ironic; and one wonders whether, in taking it as his title for the whole series of papers, he meant to suggest that most of the writers studied by him were guilty of using ' sovereign reason ' ' to thrust aside what they do not fancy '. I should not be at all surprised if he did.

L. J. Russell

Biophysik des Fliessgleichgewichts. By Ludwig von Bertalanffy.
Sammlung Vieweg, Heft 124, Braunschweig, 1953. Pp. iv + 56.
DM 4.80

In this booklet the author covers the subjects treated by him in this *Journal* (1950, **1**, 134-165) but with some extra detail and matter. It is a general study of such topics as the kinetics of open systems, their equilibria, steady states, energy exchanges, equifinality, and thermodynamics.

It is difficult for me to say much on the theses proposed, for though I entirely agree with the author on the desirability of a general theory of systems, I am not in sympathy with his method of proceeding. So it is difficult for me to be brief without being unfair. Briefly, my feeling is that a properly general theory of systems must resolutely divorce itself from actual, contemporary, physics and chemistry ; it must develop first in the world of the abstract, and must come into contact with the real world only when possessed of sufficient *internal* coherence to be able to preserve its structure, which must be rich and comprehensive enough to include *any* physical system, or any real behaviour, that may confront it.

The position, in my opinion, resembles that in the relation of geometry to real physical space. There was a time when the first essential of any geometry was that its properties should agree with those in real physical space. Physical space dominated geometry. Today, the relation is reversed ; geometry exists in its own right, much richer than physical space in content, and able to represent *any* space found to have real existence. In the same way, system theory should, in my opinion, forget about physics and chemistry until it is stronger.

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[1] The Greek phrase *λογισμῷ αὐτοκράτορι διωθεῖσθαι* is more accurately translated ' to thrust aside by arbitrary reasoning ' or ' by arbitrary judgment '. The suggestion of ' sovereign ' occurs in a note in the critical edition of Thucydides produced by Dr Thomas Arnold of Rugby in 1832, from which Crawley probably took it.

Be that as it may, many workers are convinced of the urgency of the problem of bringing to such concepts as organisation, system, and adaptation the same precision as has been achieved with curvature, continuity, and manifold. To this end the author's contribution is substantial and welcome.

W. ROSS ASHBY

The Logic of Modern Science. By J. R. Kantor.
The Principia Press Inc., Bloomington, Indiana, 1953. Pp. xvi + 356. $ 6·00

A NUMBER of scientists and philosophers are recognising today that we need to cut down the epistemological undergrowth if we do not want our thoughts to choke. Professor Kantor, as a psychologist, is perhaps more sensitive than most people to the fact that the various *-isms* in terms of which science is conventionally explained, however ingenious they may be, have become useless. He has therefore tried to give an unconventional account of the scientific enterprise, without the aid of received philosophy.

Science, after all, is an activity. The main idea put forward here is to explain the various sciences—and not a mythical universal Science—by describing the *material* on which the scientists work, the *instruments* with which the work is carried out, and the *product* in which the work results. These terms are more closely related to what we do in the laboratory than are the traditional ' universals ', ' sense data ', and so on ; and they may help us to escape from the ontological decisions that are entailed by our customary philosophical jargon. For ' material ' may mean billiard balls and electric fields and muscle tissues as well as thoughts and feelings ; that is, the term refers to things that are usually considered as either physical or mental. Similarly, the instruments are a balance and a scalpel or concepts and words ; and products are represented by a meson track or penicillin or by a sentence and a theory. In this way we might be able to overcome the dualism of mind and body which underlies all our reasoning ever since the Greeks and before ; and the view that knowledge is achieved by somehow fitting the ideas in our heads to the things outside of them, or *vice versa*, may at last be abandoned.

This approach, I think, is basically sound. And let me remark, parenthetically, that others have been and are trying it out. The Italian school around S. Ceccato have worked along these lines for some time and, in fact, employ exactly the same terminology, though Professor Kantor does not seem to be aware of it. Another strange omission, in the discussion of psychology, is that of psycho-analysis ; for Freud always insisted on keeping clear of the mind-body problem.

The author, however, does not quite succeed in his ' inter-behavioral ' analysis, as he calls it. On occasion he gets himself lost in the epistemological thicket, e.g. when he says that modern physics has become ' abstract ' while classical physics deals with ' concrete ' objects. But electrons and mesons are no whit less ' concrete ' than the particles of mechanics or the light rays of optics : they are only less familiar. Speaking generally, the level of discussion in this book is a little uneven ; it often proceeds by rhetorical rather than by logical argument (even if we take ' logic ' in the wider sense of the author's, namely, as critique of the assumptions involved in theory construction).

It would be a pity if the book were not read because of the unattractive style in which it is written. The author's manner of presenting his ideas is likely to repel most readers ; and iconoclasm does not impress us and may even arouse our hostility. Still the reader, for his part, should be prepared to meet the author's ideas without being too strongly led by his own preconceptions. The book could stimulate us into thinking anew—and, in my view, this is a not inconsiderable merit.

E. H. Hutten

Philosophical Writings : *A Selection*. By R. Descartes.
Translated and edited by Elizabeth Anscombe and P. T. Geach. Thomas Nelson & Sons, Edinburgh. Pp. lvi + 303. 12s. 6d.

A volume of selections from Descartes arranged by Professor Kemp Smith was reviewed in this *Journal* in the August Number last year. The present selections cover somewhat similar ground at half the price. The chief difference is the inclusion here of Descartes's replies to Hobbes.

The reader may be reminded that the outstanding item of interest of the previous volume was the little known discussion of the mind-body problem. Kemp Smith's selections contained letters to Arnauld on this subject, which unfortunately are missing here ; but the present volume prints Princess Elizabeth's letters on it, which the other merely summarised.

J. O. Wisdom

ABSTRACTS

Dialectica, 1954, **8**, No. 3

A. Landé, ' Quantum Indeterminacy, a Consequence of Cause-Effect Continuity '

Under the general principle of continuity it must have physical significance to introduce a continuous scale of possible degrees of equality between two states A and B of an object, beginning at identity, $A = B$ with equality degree $q = 1$, and ending at total inequality, $A \neq B$ with equality degree $q = 0$. The intermediate case of a fractional equality, $A \sim B$ with q between 1 and 0, must signify a kind of ' fractional separability ', meaning that a B-passing filter will answer to the state A sometimes with Yes and sometimes with No, at a statistical ratio which is a probability ratio for the individual test. Such a ratio can never be reduced to hidden determinism. The same applies to the statistical ratios of an ordinary classical game of chance, which depends on symmetry conditions only. The rules of the quantum game are different from those of classical theory. In particular, there is a superposition of probability amplitudes Ψ. The latter can be seen as the natural answer to the requirement that there be a general interrelation law between the various probabilities which must be symmetric in all states. In this way one can reduce the concepts and formal rules of quantum theory to simple and immediately acceptable physical postulates.

Colin M. Turbayne, ' Berkeley and Russell on Space '

The interpretation of Berkeley's philosophy depends upon the meaning given to his phrase, ' in the mind '. Berkeley was careful to state that this was a metaphor. All he meant by it was, ' perceived ' or ' comprehended '. However, in spite of his acute linguistic sense, Berkeley never completely freed his thinking from the physical reference of such an expressive metaphor ; that is, he often wrote as though he took ' in the mind ' literally.

Accordingly, Berkeley wrote from two different standpoints that are appropriately characterised by Russell's distinction between perceptual and physical space. He avowed only the former. It corresponds with his commonsense realism. Acceptance of the latter, although disavowed, is presupposed. This is most evident in Berkeley's treatment of the Molyneux problem which influenced his theory of vision and, through that, his mature philosophy.

Haskell B. Curry, ' Theory and Experience '

This is a statement of opinions concerning the relation of formalised theories to experience which were prepared for discussion at the third ' Entreteus de Zurich ' held in April 1951. The opinions are not supported by a detailed argument. Informal definitions are proposed for ' contensive validity ' and ' acceptability ' ; their relations to one another and to consistency are discussed briefly. It is stressed that acceptability is always an empirical matter, even for theories of logic ; and that questions of simplicity may play an important rôle in it.

E. Specker, 'Die Antinomien der Mengenlehre'

After a brief explanation of the mathematical notion of set it is shown that the naïve theory of sets is inconsistent (Russell's antinomy, for example). The hypotheses which lead to these antinomies are analysed and some of the suggestions that have been made to avoid them are discussed.

Dialectica, 1954, 8, No. 4

Henry W. Johnstone, Jr., 'Some Aspects of Philosophical Disagreement'

This paper is an attempt to develop some of the consequences of the belief that there is philosophical disagreement. If the disagreement is real, it cannot be overcome by means of any technique. Perhaps, then, it is unreal. But this thesis has never received a satisfactory formulation. Some attempted formulations, such as the positivistic, the analytic, and the dialectical, are critically surveyed. In addition, two theories that presuppose the reality of philosophical disagreement are examined : the intuitionist and the critical. Finally, a view is proposed that is intended to do justice to the insights of all the doctrines criticised, while avoiding their deficiencies.

W. Pauli, 'Naturwissenschaftliche und erkenntnistheoretische Aspekte der Ideen von Unbewussten'

In this article I was guided outside my special branch of science, by coincidences of the *sense* of the ideas occurring almost simultaneously in different sciences : 'correspondence', 'complementary pairs of opposites' and 'wholeness' appear independently both in physics as well as in the ideas of the unconscious. The 'unconscious' itself has a certain analogy to the 'field' in physics and both are shifted by an observational problem, outside the range of visualibility into the paradoxical. Although in physics one does not speak of 'archetypes' which reproduce themselves, but of 'statistical laws of nature with primary probabilities', both formulations meet in the tendency to amplify the older more narrow idea of 'causality (determinism)' to a more general form of 'connections' in nature, toward which the psycho-physical problem also points. This way of consideration leads me to the expectation, that the ideas on the unconscious will not be developed further in the narrow frame of its therapeutic applications, but that their junction with the general stream of the natural sciences of the phenomena of life will be decisive for them.

C. A. Meier, 'Projektion, Übertragung und Subjekt-Objektrelation in der Psychologie'

It is shown that the well known psychological terms 'projection' and 'transference' have to be examined more carefully, as they cover cases where the far-reaching psycho-physical problem and the problem of 'synchronicity' (C. G. Jung) is involved. Whereas the original concept of projection is asymmetrical and would not explain certain phenomena in psychology, a new, symmetric schema explains not only hitherto neglected aspects of these phenomena, but, in addition, seems to shed light on the still unexplained, yet now well established facts of ESP, etc. In

the theory an objective constellating factor is posited which accounts for the similarity between the patterns of subject and object in case of projection and transference.

M. Gex, ' Les évolutionnismes contemporains de Gustave Mercier et du Père Teilliard de Chardin '

On assiste de nos jours à l'édification de vastes synthèses philosophiques sur des bases scientifiques : Gustave Mercier et le Père Teilhard de Chardin, entre autres, s'inspirent, dans leurs évolutionnismes spiritualistes, de l' *Evolution créatrice* de Bergson.

L'homme est un moyen donné à la nature pour se dépasser, il est devenu un facteur conscient et important de l'évolution, et celle-ci ne peut s'expliquer qu'en fonction de lui. L'homme n'apparaît plus comme un accident incompréhensible, car il est fortement intégré dans un univers en devenir, en étroite solidarité avec le reste des vivants, animaux et végétaux.

Cette vision objective de l'homme ne méconnaît nullement son intériorité, son caractère d'être réflexif qui le distingue de tous les autres êtres et lui confère une valeur éminente. Ainsi se trouve restaurée une philosophie harmonieuse qui ne sacrifie pas, comme l'existentialisme, le monde objectif de la science au monde intérieur de la conscience, mais qui, selon la tâche essentielle de la philosophie, cherche comment ces deux mondes s'articulent l'un à l'autre.

Methodos, 1954, 6, No. 24

F. Crahay, ' Logique formelle, empirisme logique et vérité '

Those who claim formal logic to be plainly independent of any philosophical commitment often seem to overlook (1) the historical *facts* of co-operation between formal logic and logical empiricism, (2) the cases of more or less implicit shifting from a *de facto* to a *de jure* situation.

Three subcases are here examined of an ' explaining away ' argumentation which, whether directly or indirectly, contributes to bestowing undue prerogatives upon formalisation. Objections are raised especially against some points of Ayer's discussion towards reducing the concept of truth in such a way that it can be either dispensed with or readily assumed by that kind of uncritical epistemology which gravitates towards formalisation and formal systems.

Béla Juhos, ' Deduktion, Induktion und Wahrscheinliekkeit '

This paper investigates the logico-syntactical criteria of *deductive conclusions* and of *probability-conclusions.* Both kinds of conclusions can be entirely characterised by formal-syntactical criteria alone, and hence are analytic. Deductive conclusions provide only one kind of relation between premisses and concluding sentences, the tautological implication, whereas probability-conclusions allow infinitely many kinds of ' relations of probability-consequences ' between given premisses and concluding sentences. In practice we have to select one of these relations arbitrarily. Relations of probability-consequences can also be defined as mathematical probability-functions. ' *Inductive-conclusions* ', on the other hand, which are not analytic, are selected by practical rules for which no logical reason can be given.

ABSTRACTS

Louis O. Kattsoff, ' Brains, Thinking and Machines '

This is a semantical consideration of the meaning of words like ' thinking ', ' brains ', and their significance in sentences in which the word ' machine ' also occurs. The significance of the problem arises because of the tendency to make statements like ' machines are brains ', ' machines think ', and ' machines learn '. The meaning of expressions like ' machines think ', obviously depends upon the meaning of the words ' machine ' and ' think '. The misleading thing is the emotive connotation which is attached to these sentences. Analogies between machines and brains neither prove nor disprove that the two are species of the same genus.

Bernard Peach, ' Obligation, Ability and Disposition : An Example of two Methods of Ethical Analysis '

This paper is a discussion of obligation, ability and disposition making use of historical material from the writings of Francis Hutcheson and some techniques of contemporary analysis, particularly as they appear in W. K. Frankena's ' Obligation and Ability ' (*Philosophical Analysis*, edited by Max Black, Ithaca, 1950).

Jason Xenakis, ' Meaning '

The philosophers of meaning try to answer a question (' What is meaning ? ') without realising that they must first get straight about another one (' What usages of this or that meaning-word are relevant to the meaning-question ? '). Hence also their endless and futile disputes ; for one party bases his view of meaning on *x* usages of ' meaning ' (or of ' to mean ', etc.) and the other on *y*. I then tackle the Meaning-Ownership Question, and distinguish meaning from referring, asserting, describing, truth/falsity, compatibility/incompatibility, all of which I put in the employment of meaning-owners. Next, I deal with such notions as definition, analyticity, etc. ; I dispose of alternative views of meaning, including the synonymy and use views, and psychological and semantical views ; and conclude that meanings are written or unwritten rules governing our employment of words, phrases, and the like. I also tackle the question of how meanings originate and change owners, and distinguish meaning-rules from other sorts of rules, and talking from other rule-governed activities such as playing a game.

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

(*a*) Books Received for Review

Inclusion of books in this list does not preclude their being reviewed in later issues

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Beth, E. W., *Les fondements logiques des mathématiques,* Gauthier-Villars, Paris, 1955, pp. xv + 236

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Fevrier, Paulette, *Déterminisme et indéterminisme*, Presses Universitaires de France, 1955, pp. xxii + 250

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Hoijer, Harry (Ed.), *Language in Culture : Conference on the Interrelations of Language and other aspects of Culture*, Chicago University Press, 1955, pp. xi + 286, 34s.

Jammer, Max, *Concepts of Space*, Oxford University Press, 1955, pp. xvi + 196, 30s.

Knode, Jay C., *An Intellectual Primer*, Philosophical Library, New York, 1955, pp. vii + 88, $2.50

Körner, S., *Kant*, Penguin Books, Harmondsworth, 1955, pp. 230, 2s. 6d.

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